PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES
Volume 3 JANUARY 15, 1917 Number 1

INFERENCES CONCERNING AURORAS

By Elihu Thomson
GENERAL ELECTRIC COMPANY, WEST LYNN, MASSACHUSETTS
Read before the Academy November 14, 1916

It seems doubtful if any extensive auroral display has occurred without coincident existence of exceptional areas of disturbance on the sun. We may assume that at such times great jets or streams of electrified matter (electrons perhaps) akin to cathode rays in a vacuum, are projected with high velocities outward, and that occasionally some of these jets cross the earth's orbit or pass near to it, being when leaving the sun in a general radial direction, bent backwards for obvious reasons.

That electrified matter in a vacuum does move in jets or streams for indefinite distances is a fundamental fact. Moving charges of the same name in paths, straight or curved, act like parallel currents and attract one another, the more as their velocity is greater, until the static repulsion of such like charges sets a limit to further approach. It is possible that the space around the sun may be crossed by many such jets or streams of electrified matter moving at very high velocities into the vacuous space. The coronal streamers may be the visible composite effects of the projected jets. Such electrified jets may act inductively by proximity to the earth or directly by conduction of electricity to the earth's outer atmosphere.

In the present paper, however, it is hoped to prepare the way for further study by pointing out certain physical facts regarding the relations of auroral phenomena to the earth and its atmosphere: to locate and give direction to the streamers seen in auroras; and to explain the nature of the so called auroral arch, the zenith crown, and other characteristics. It is believed that the following propositions may be shown to be true.

1. Streamers seen in auroras, singly or in composite masses, are in reality vertical, or approximately so, to the earth's surface, nearly parallel when adjacent, and only slightly divergent even when miles apart; the divergence being due to curvature of the earth's surface.

2. In any aurora, the streamers appear to be located in bands or zones more or less wide in latitude extending generally in east and west direction, or forming belts or zones between parallels of latitude in which the streamers extend vertically upward like trees in a forest.

3. In some rare auroras the vertical streamers are closely limited to a narrow belt of latitude; sometimes only 2 or 3° or even less, in width north and south, while the east and west extent of the narrow belt may be very great.

4. In wide spread auroras the belt of vertical streamers may cover great extents of latitude and extend east and west unlimited distances. This appears to have been the case in the recent great aurora of August 26, 1916.

5. The curvature of the so called auroral arch is an optical effect of perspective, slightly added to by the curvature of the earth. The appearance of folded curtains of streamers is a similar effect of superposition and perspective when the active band or zone covers many degrees in latitudes. It is probable that the lower ends of auroral streamers have about the same height in the earth's atmosphere; a layer from which they stream upward to heights which vary in different displays or even in the same display. This layer probably exists at a height of about fifty miles and conducts laterally or horizontally, thus distributing the electricity discharged from it into the streamers.

6. The convergence of long streamers toward the zenith seen in the greater auroras, is purely an optical effect of perspective, the streamers being vertical.

7. The so called zenith crown is in reality due to bundles of streamers, nearly vertical but seen on end. They appear as patches of shifting light in or near the zenith, sometimes surrounded by apparently converging streamers from the north, east, and west, and even from the south; converging in appearance only.

8. The convergence of streamers is of the same nature as the convergence of straight parallel railway tracks in the distance, or better, the apparent convergence toward the sun, of the sunbeams seen in dust-laden air, when the sun itself is obscured by a small irregular cloud, or is back of a broken mass of clouds.

In April, 1883, at near the middle of sunspot period and coincidently with the occurrence of an enormous sunspot, there was perhaps the

greatest auroral display seen in our latitudes for more than half a century. Telegraph lines from east to west could not be operated owing to arcing at the keys. The display as witnessed by me, was characterized by colored streamers passing upward from all around towards the zenith from north, east, west and south. The coloring gave me the first clue to the true relations existing. In the north the sharply defined streamers beginning their appearance low in altitude with the usual greenish auroral light, shot, or spread upward with changes of tint, finishing at their upper ends in a deep crimson. The light patches of the zenith crown went through the *same succession* of coloring constantly, each time ending in deep crimson. This fact suggested in the strongest way that in the crown one was observing bundles of streamers on end, but of the same character as all the other streamers in the field of view. Great masses or broad bands to the east and west though steady were colored likewise. These dense masses in the east and west were unquestionably due to the composite effect of great numbers of streamers superposed in the line of sight through great distances to east and west; the colors less pure blurred by distance and failure of exact superposition. Other observers at places far to east and west of my position might observe zenith crowns but composed of other streamers seen on end. A necessary consequence of the display presenting the same appearance to observers in many places far apart, up to hundreds of miles east and west, and through a zone north and south, is that the streamers everywhere were in reality vertical, or approximately so, to the earth's surface. In Chicago, a distance west of about eight hundred miles, this aurora was described in the same way, the appearances coinciding with my observations. The effects being so similar in places far apart, the only inference possible is the one stated, namely, nearly vertical streamers wherever seen.

In this view observations upon subsequent auroras could be made more intelligently than before. But auroral displays of great magnitude are rare in our latitudes, especially such as are so far south as to cause a belt of streamers directly over the place of observation giving the zenith crown. While several highly instructive displays occurred meanwhile, one on September 29, 1908, gave streamers converging from all directions joining the crown in the zenith which had the usual characteristics. My note of it says "The rapid procession of streamers and the waving curtains make this display quite exceptional; though far inferior to that of April, 1883." On August 26, of this year (1916) there occurred a display second only in my experience to that of April, 1883, but without the remarkable coloring of the display of that year.

It was observed by me in the Adirondacks, and served to confirm the inferences drawn from the earlier displays.

But on August 28 there occurred a simple type of aurora which I had only seen on two former occasions. It may be said to contain a single element only. It consisted of a single band a belt of light, estimated at from $1\frac{1}{2}°$ to 2° wide extending across the sky due east and west, from horizon to horizon and passing through the zenith. It lasted for an hour or a little more. At the zenith and for 2 or 3° east and west thereof, the band was broken into patches which shifted, faded and were replaced by others of different shapes and positions. A friend observed the same appearance in Maine hundreds of miles to the east on the same night.

The explanation of this simple type of aurora is not difficult when made in accordance with the ideas here presented. No streamers were seen as such. At the zenith they were directly over the observer, and though present were seen only on end as small patches of light constantly changing: while in the extensions or narrow bands to east and west from the zenith the luminous streak was a composite of super-posed streamers in the same latitude as the observer, which streamers overlapped each other all the way down to the apparent horizon along the overhead arch east and west. They were like the palings of an extended fence located far above the observer. He would see the separate palings, directly above him but only on end. Otherwise they overlap and obscure each other or one another. To appreciate fully the reality, the observer is better placed if the auroral arch when, as in this case, very narrow is not directly overhead. The usual auroral arch is the location of the lower ends of the vertical streamers extending upward from it. This location would appear from the work of Störmer and others to be in a layer of atmosphere about fifty miles high. This layer on which the feet of the streamers may be said to rest is probably a conducting layer like the partial vacuum in a Geissler tube.

The so called auroral arch appears as an arch or curve merely because of perspective vision, just as a searchlight beam or a long straight cloud appears bent when crossing the sky, or as a cloud layer, known to be horizontal appears above us as a great inverted bowl.

The streamers in the aurora of August 26, were, as in the aurora of 1883, directed towards the zenith crown from all sides. The appearance of the whole aurora when at its height, and looking upwards towards the zenith was as if one were looking into a truncated hollow cone from below its base, with the slant sides composed of ribs of light; these ribs as well as the blunt apex, constantly changing, shifting, fading, and

returning and traversed by waves of light of varying intensity. These appearances were not, of course, peculiar to the particular post of observation but were observed from points far east and west without substantial differences. A vivid description by Dr. C. C. Nutting is found in *Science* for October 6, 1916, pp. 496 and 497. See also *Science* of November 10 to November 17 and December 8, 1916, for other letters concerning this great aurora.

The belt covered by this display was evidently of very great extent east and west and spread far to the north in latitude. One is compelled to recognize that observers far apart seeing the same appearances are looking up between nearly parallel and vertical streamers, seeing those on end as a zenith crown when directly above them; while laterally they are superposed by being back of one another at varying distances. The same auroral appearances are possible to be seen alike at different places simultaneously, *only* when a system of *vertical streamers exists*.

Let us for illustration assume an extended horizontal flat surface and that there be erected above it a set of vertical and very long rods in a vertical plane extending east and west, the rods being spaced apart like the paling in a long straight picket fence. An observer on the plane faces north looking towards the rods or paling the lower ends of which are high above his position, appearing say at an altitude of 60° in the north direction. The lower ends will now appear to lie east and west in an arch of curve convex upward owing to diminished angle of vision with distance and the rods or palings will appear to converge upward if long enough almost to the observer's zenith, much foreshortened; the whole effect being that of perspective. Removing the point of observation further to the south, the middle point of the arch drops more and more, the arch becomes flatter, and the vertical rods appears less foreshortened and longer, while still converging towards the observer's zenith. These varied appearances, modified to a minor degree by the earth's curvature, are just what are seen in auroras. If the vertical rods are spaced apart irregularly, increased in number and spread into a band so that they do not lie exactly in a vertical plane east and west, but in an arrangement like a long strip of forest extending east and west and of considerable width north and south (an arrangement corresponding to a belt or zone of streamers instead of a single line), the analogy to the auroral arrangement at any instant is much closer.

It can readily be seen that the recognition of the vertical relation of streamers to the earth's surface and the nearly constant level of their lower ends simplifies to a great extent the study of auroras, particularly

the determination of the total height reached by them, curvature of the earth being allowed for. The effect of this curvature will be less the higher in altitude the auroral arch, or the nearer it is to being overhead. The streamers are often observed to rise from the arch first as short streamers, gradually developing and extending upward until their upper ends are a few degrees from the zenith. When they originate in an arch which is of low altitude and extend nearly to tbe zenith, as they appear to do in the greater displays, their length must be hundreds of miles, possibly in some instances reaching one thousand or more. It would appear that no limit can be set for their possible height. In most auroras, however, the visible extent of the streamers is more limited. A low altitude auroral arch implies great distance north from the observer and for a given actual streamer length a less apparent height or length. Paulsen's class of auroras without streamers may mean either that the streamers are too short and too many to be noted separately, or that the electrification is too feeble for their development, the observed luminous glow being due to flow of current in the conducting layer itself, an arch forming horizontally, but without outward projection.

If our assumptions are approximately correct the arch of an aurora, if located farther north than about 600 miles from an observer will be below his horizon but the streamers extending upward from it if long enough may be seen. Auroras far north of this will probably be invisible or be seen merely as a luminous glow well down on the northern horizon. When the breadth in latitude of the auroral zone is great and the display is seen from the south, the streamers may overlap or be arranged in apparent curtains or folds, the lower ends of the streamers being in that case at varying apparent altitudes above the northern horizon even when in the same general line of view. In such case they may be superposed in the line of sight and therefore be composite, or increased in apparent length owing to imperfect superposition in their lengths with respect to those back or front of them. It is believed that these and like considerations will suffice for the explanation of observed appearances of auroras in spite of their great variety.

The direction of streamers, as indicated, being vertical to the earth's surface, is suggestive of electrical discharges, ions or electrons projected outward into space from the conducting layer of our atmosphere; discharge into space in which the mean free path is unlimited. These discharges might have their origin in a charged conducting layer in which the potential suddenly rose to a critical value, or might be brought about inductively by the presence in the space around the earth of

opposite charges possibly arriving in great jets from the sun. Further the charges might be communicated to our air from such jets followed by discharge into space beyond.

A sudden uplift of a highly charged thin air layer itself in waves or ridges might easily disturb the normal potential distribution and precipitate discharges from the crests. Atmospheric currents or displacements may cause such uplifts as when a colder stream strikes down under a warmer charged layer.

A set of vertical streamers would deflect a compass needle on the earth's surface one way or the other depending on whether such streamers exist to the north or to the south of the position of the compass. Observations seem to confirm this but more work is needed. The direction of the compass deflection would determine the direction of the virtual streamer currents to which the deflection was due. Aside from these and other considerations, the effort has, however, been to present in this communication a rational theory which will at least enable a proper conception of the actual space relations of the visible portions of an aurora in relation to the earth's surface to be obtained; and to place on record ideas which through many years of consideration by the author have seemingly received at each appearance of an aurora with streamers, repeated confirmation.

While I have in former papers, as in "Thoughts on Osmical Electricity," an address before the Franklin Institute of Pennsylvania (December 19, 1893) and notably in an address on "Atmospheric Electricity" delivered at Princeton University October 21, 1909, and published in *Science*, December 17, 1909, pp. 857 to 869, given a brief sketch of some of the views presented, particularly the outward direction of the streamers, later observations have served to provide cumulative evidence and extend their application.

APPLICATION OF THE LAWS OF ACTION, REACTION AND INTERACTION IN LIFE EVOLUTION

By Henry Fairfield Osborn

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK CITY

Read before the Academy, November 13, 1916

Since 1893 I have been working upon the interrelations of the various so-called factors of evolution and have published a series of studies of this subject. In 1908 I presented before the American Society of Naturalists an exposition of a law which I termed "the law of the four inseparable factors of evolution," these factors being regarded as the

sum of the forces inherent in environment, in individual development or ontogeny, in race development or heredity, and in natural selection. This conception was worked out more fully in 1912 and published in the form of a preliminary statement, as "Tetraplasy, the Law of the Four Inseparable Factors of Evolution." During the past two years I have been engaged in working out the aspects of this law from the standpoint of physics[1] and chemistry, that is, interchange of energy, in preparation for the Hale Lectures before the National Academy of Sciences on "The Origin and Evolution of Life upon the Earth." I perceive that it was an error to regard Selection as one of the four inseparable factors because it is not a form of energy. Consequently the law should be restated in the following terms:

In each organism the phenomena of life represent the action, reaction, and interaction of four complexes of physico-chemical energy, namely, those of (1) the inorganic environment, (2) the developing individual (cytoplasm and somatic chromatin), (3) the germinal or heredity chromatin, (4) the organic environment. Upon the resultant actions, reactions, and interactions of each organism Selection is constantly operating whenever there is competition with corresponding actions, reactions, and interactions in other organisms.

I believe this to be the most fundamental biologic law which can be expressed from our existing knowledge. It is in part an application to life phenomena, first, of Newton's third law of motion,[2] in the light of which physicists have given the full dynamical meaning to the modern laws of thermodynamics, second, of the laws of thermodynamics, and, third, of Darwin's law of Selection as developed by Weismann, Roux, Osborn, and others in modern biology. The reign of the laws of motion, including the motion of electricity, and of thermodynamics in the life processes follows as a necessary consequence of our modern physio-chemical interpretation of many of the phenomena which were formerly regarded as vitalistic.

This law as operating between two or more organisms may be clearly expressed in the following scheme.

Organism A		*Organisms B—X*
Physico-chemical Actions, Reactions, and Interactions of the Lifeless Environment	Natural Selection (Competition, Survival, Elimination, Cessation of Selection, Reversed Selection, Organic Selection, etc.)	Physico-Chemical Actions, Reactions and Interactions of the Lifeless Environment
Developing Organism Cytoplasm and Chromatin		Developing Organism Cytoplasm and Chromatin
Heredity Chromatin		Heredity Chromatin
Life Environment		Life Environment

Physico-chemical actions and reactions, which, so far as known, follow the laws of conservation of energy, are the chief phenomena observed in modern physiology, as set forth in such works as Loeb's "Dynamics of Living Matter." Through catalysis many of the actions and reactions are known to send off chemical messengers which are among the chief means of interaction in different parts of the organism. There are in animals other means of interaction, such as the enzymes, the secretions of the duct glands, the internal secretions of the ductless glands, and the nervous system. These interactions do not follow the laws of conservation of energy; they work at a distance, and the effects do not balance the causes. Certain phenomena of interaction, in which chemical messengers of various kinds coördinate the actions and reactions of the organism, are now understood both in physiology and pathology as coördinating, correlating, accelerating (hormones), and retarding (chalones), as well as balancing growth and development.

The nature of the actions, reactions, and interactions between the physical environment, the development organism, and the life environment are relatively well understood. It remains to be determined what relations these actions, reactions, and interactions respectively have to the physico-chemical processes in the somatic and in the germinal chromatin. There are some grounds for the hypothesis of Cunningham that some of these chemical messengers affect in a similar manner the germinal and bodily chromatin, but this subject is very obscure at present.

[1] I am indebted to my colleague M. I. Pupin for valuable suggestions in formulating the physical aspect of these principles. He regards Newton's third law as the foundation not only of modern dynamics in the Newtonian sense but in the most general sense, including biological phenomena. The second law of thermodynamics started from a new principle, that of Carnot, which apparently had no direct connection with Newton's third law of motion. This second law, however, in its most general form cannot be fully interpreted except by statistical dynamics, which is a modern offshoot of Newtonian dynamics. With regard to the first law of thermodynamics, it is a particular form of the principle of conservation of energy as applied to heat energy. Helmholtz, who first stated the principle of conservation of energy, derived it from Newtonian dynamics.

[2] "I. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon."

"II. The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed."

"III. To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts."

"Newton's Principia; the Mathematical Principles of Natural Philosophy, by Sir Isaac Newton," translated into English by Andrew Motte, publ. Daniel Abbe, New York, 1848, pp. 83–84.

THE RESISTANCE OF METALS UNDER PRESSURE

By P. W. Bridgman

JEFFERSON PHYSICAL LABORATORY, HARVARD UNIVERSITY

Communicated by E. H. Hall, November 27, 1916

In this note are summarized many of the most important results of an extended series of measurements on the resistance of metals under hydrostatic pressure. A more detailed account of the experiments has been offered for publication in the *Proceedings of the American Academy of Arts and Sciences*. The pressure range of this work is from 0 to 12,000 kg., and the temperature range from 0° to 100°C. The most extensive previous measurements have been made by Lisell and Beckman,[1] who made all their measurements at 0° over a pressure range of 3,000 kg.

This investigation includes 22 metals, embracing nearly all the common metals obtainable in the form of wire. Special precautions were taken in most cases to insure the greatest possible purity; the temperature coefficient of resistance at atmospheric pressure affords an indication of the probable purity. Of the metals above, Sn, Cd, and Zn were Kahlbaum's 'K' grade, Tl and Bi were prepared by electrolysis and were of high purity, Pb, Ag, Au, Cu, Fe, and Pt were of exceptional purity, and the others are probably not better than of high commercial purity except Al, which was much better than ordinary.

The results are summarized in the table. Two kinds of pressure coefficient are listed: 'instantaneous coefficient' and 'average coefficient'. The 'instantaneous coefficient' is the value of the derivative $\frac{1}{w}\left(\frac{\partial w}{\partial p}\right)_t$, where w is the observed resistance at the pressure and temperature in question. The 'average coefficient' between 0 and 12,000 kg. is the total change of resistance between 0 and 12,000 kg. divided by 12,000 and by the resistance at atmospheric pressure at the temperature in question.

The effect of pressure is to decrease the resistance of all metals except Bi and Sb, the resistance of which suffers a comparatively large increase. The anomalous behavior of Bi was known before, but that of Sb is new. The Sb was used in the form of extruded wires. The magnitude of the effect for normal metals varies from 12% to 0.8% for 10,000 kg., but Te forms a notable exception.

It is apparent from the table that the relative change of pressure coefficient with temperature is much less than the relative change of

resistance itself; the direction of change may be either an increase or a decrease. Another statement of this fact is that the temperature coefficient changes very little with pressure; this is shown in the second and the third columns of the table. This is perhaps surprising when it is remembered that the pressures used here are in many cases great enough to compress the metal to considerably less than its volume at O°Abs. at atmospheric pressure. The instantaneous coefficient de-

TABLE

METAL	AVERAGE TEMPERATURE COEFFICIENT 0 TO 100°		PRESSURE COEFFICIENTS					
			Instantaneous coefficient at 0°		Instantaneous coefficient at 100°		Average coefficient 0 to 12,000 kg.	
	At 0 kg.	At 12,000 kg.	At 0 kg.	At 12,000 kg.	At 0 kg.	At 12,000 kg.	At 0°	At 100°
In	+.00406	+.00383	−.0₄1226	−.0₄1016	−.0₄1510[3]	−.0₄1072[3]	−.0₄1021	−0.₄1131[3]
Sn	447	441	₄1044	₅936	₄1062	₅973	₅920	₅951
Tl	517	499	₄1319	₄1180	₄1456	₄1200	₄1151	₄1226
Cd	424	418	₄1063	₅837	₄1106	₅887	₅894	₅927
Pb	421	412	₄1442	₄1220	₄1483	₄1237	₄1212	₄1253
Zn	416	420	₅540	₅425	₅524	₅407	₅4700	₅4544
Al	434	435	₅416	₅365	₅397	₅373	₅3815	₅3766
Ag	4074	4069	₅358	₅321	₅355	₅331	₅3332	₅3362
Au	3968	3964	₅312	₅286	₅304	₅292	₅2872	₅2918
Cu	4293	4303	₅201	₅179	₅184	₅175	₅1832	₅1770
Ni	4873	4855	₅158	₅142	₅163	₅156	₅1473	₅1575
Co	3657	3676	₆941	₆814	₆755	₆704	₆873	₆726
Fe	6206	6184	₅241	₅218	₅247	₅230	₅2262	₅2353
Pd	3178	3185	₅198	₅190	₅189	₅187	₅1895	₅1863
Pt	3868	3873	₅1975	₅181	₅190	₅182	₅1870	₅1838
Mo	4336	4340	₅133	₅126	₅130	₅125	₅1286	₅1265
Ta	2973	2967	₅149	₅139	₅153	₅147	₅1430	1486
W	3219	3216	₅128	₅121	₅130	₅123	₅1234	₅1258
Mg	390[1]		0₅55				0₅55	
Sb	473	403	+ 0₄1220	+ ₄1064	+ ₅768	+ ₅723	+ ₄1220	+ ₅768
Bi	438	395	+ 0₄154	+ ₄213	+ ₄152[4]	+ ₄1895[4]	+ ₄2228	+ ₄1980[4]
Te	−.0063[2]		−.0₄129					

[1] 0° to 20°, [2] 0° to 24°, [3] extrapolated from 50°, [4] extrapolated from 75°.

creases with rising pressure, as is natural, but it is at first sight strange that the percentage decrease is in nearly all cases less at the higher temperatures.

The numerical results of the above table are not in particularly good agreement with the previous results of Lisell and Beckman.[1] Suggestions as to the reasons for the discrepancies may be found in the detailed paper. Beckman has made extended application of his results to a theory of the pressure effect recently put forward by Grüneisen,[2] and

has come to the conclusion that Grüneisen's theory can not be more than a first approximation. This conclusion will not be altered by using the values of the table above instead of those of Beckman.

The theory of Grüneisen is incomplete in the sense that it gives the pressure coefficient of resistance in terms of the temperature coefficient as well as several thermodynamic constants. I hope to show at some length elsewhere that both the temperature and the pressure coefficient of resistance may be calculated with better agreement than by Grüneisen's formula by putting the proportional change of resistance in any direction equal to twice the proportional change of average amplitude of atomic vibration. This is capable of theoretical explanation on the ground that when the atoms are at rest the electrons pass freely from atom to atom, but when the separation of the atoms by haphazard heat agitation becomes too great, the electrons encounter difficulty in jumping from atom to atom.

The expenses of this investigation were in large part met by generous appropriations from the Bache Fund of the National Academy of Sciences, and from the Rumford Fund of the American Academy of Arts and Sciences.

[1] Lisell, E., Inaug. Dis., Upsala, 1902; and Beckman, B., Inaug. Dis., Upsala, 1911; *Ark. Mat. Astr. Fys.*, **7**, 1912, No. 42 (1–18); *Ann. Physik., Leipzig*, **46**, 1915, (481–502) and (931–941); *Physik. Zs., Leipzig*, **16**, 1915, (59–63).

[2] Grüneisen, E., *Berlin, Verh. D. physik. Ges.*, **15**, 1913, (186–200).

THE RATE OF DISCHARGE OF CENTRAL NEURONES

By Alexander Forbes and W. C. Rappleye

LABORATORY OF PHYSIOLOGY, HARVARD MEDICAL SCHOOL

Communicated by W. B. Cannon, November 27, 1916

The frequency of nerve impulses discharged from the central nervous system in voluntary and reflex contraction of the skeletal muscles presents a problem concerning which great difference of opinion is found among investigators. Piper, studying the electrical disturbance with a string galvanometer, has shown that in human muscles a fairly regular series of action currents with a rhythm of about 50 per second accompanies voluntary contraction. He inferred from this that the central nervous system sends to the muscle 50 impulses per second.

Buchanan, chiefly on the basis of experiments on frogs, reached the conclusion that the observed rhythm is not that of the motor nerve impulses but is dependent on the condition of the muscle itself.

By applying the principle of Buchanan's experiments to human muscles we have obtained results which confirm her main conclusion. Electrodes connected with a string galvanometer were so applied to the skin as to lead off the action currents of the first dorsal interosseous muscle lying close under the skin between the thumb and forefinger. Records were made in this way with the muscle in voluntary contraction at normal, subnormal and supernormal temperatures. Lowering of temperature was produced by immersing the hand and most of the forearm in water as cold as could readily be borne (6° to 7°C)., for about fifteen minutes; heating was produced by immersion in water at 45°C. for a somewhat shorter time. By immersion to a point above the elbow the forearm flexor muscles were treated and investigated in the same way. An unmistakable and fairly marked decrease in the frequency of muscular action currents results from this process of chilling; and an increase, which, as is to be expected, is less marked, results from heating. As nearly as can be judged from the somewhat irregular oscillations, the average action current frequencies at the three temperatures studied were in the case of the interosseous muscle as follows: cold 39 per second, normal 56 per second, hot 63 per second, in the case of the forearm flexors the corresponding estimates are, cold 36, normal 48, hot 50. We claim no strict quantitative accuracy for these figures, but the uniformity of the estimates justifies the conclusion that there is a real change of rhythm correlated with change of temperature. The temperature of the body as a whole was shown to undergo no significant change during the experiments.

The result apparently leads to the conclusion that the observed electrical muscle rhythm is not that of central innervation. It is difficult to conceive how a change in temperature in the muscle could alter the frequency of discharge of impulses from the ganglion cells whose temperature remains constant. But each propagated disturbance in a muscle fiber must depend on stimulation by a nerve impulse. Nerve impulses must therefore be so distributed in time as to render possible various frequencies of response such as those observed, depending on the temperature of the muscle. This demands a higher frequency of nerve impulses than that observed in the muscle rhythm. A careful study of the most perfectly rhythmical oscillations in our records leads us to estimate that there must be at least 300 nerve impulses per second to account for the muscle rhythms shown to be possible at various temperatures.

Piper's strongest argument against Buchanan's view of muscle rhythm which we here substantiate, is that the frequency of stimuli applied

to a motor nerve can be raised as high as 300 per second and a corresponding action current rhythm will still appear in the innervated muscle. If the muscle can respond separately to as many as 300 nerve impulses per second, he argues that it will not respond as slowly as fifty times per second unless it is stimulated only fifty times per second.

An explanation of the paradox is found in an analysis of the series of events involved in the excitation of a muscle through its nerve, in the light of certain functional properties shown by Lucas and Adrian to be essentially common to the two tissues. In either tissue a response is followed by a brief 'refractory period,' at first absolute when the tissue cannot be excited, then relative when the tissue can only be excited by a stronger stimulus than usual, and when the response is of subnormal magnitude. During the relative refractory period the excitability and the magnitude of response both return gradually to normal. At all times, including the refractory period, the magnitude of response in nerve is independent of the strength of stimulus and dependent only on the state of the tissue. The refractory period in nerve, though similar to that in muscle is much shorter.

If we assume that the stimulating effect of the nerve impulse on muscle is correlated with its magnitude as shown by other criteria, then we find no conflict between the ability of muscle to respond separately to 300 impulses per second and its response at a slower frequency to nerve impulses of a higher frequency. For as each nerve impulse is made to come earlier in the nerve's refractory period which was due to the preceding impulse, its magnitude is reduced till, coming at so early a stage in the refractory period of the muscle, it fails to excite it. One or more nerve impulses will be ineffective following each that is effective. The higher the nerve frequency the weaker will be each impulse and the more must the muscle recover to be again excited. Thus after the critical frequency is reached, the higher the nerve frequency the slower the muscle rhythm. In this respect we depart from Buchanan's view to the extent of supposing an indirect dependence of muscle rhythm upon nerve rhythm.

In view of this analysis, since the nerve rhythm evidently does not correspond with the muscle rhythm, it must have a higher frequency than the critical value (300 or more per second) below which the muscle can follow the nerve rhythm; otherwise the muscle rhythm would correspond with it.

Inferences based on these considerations together with observations of Lucas, Adrian and Garten, lead us to suppose that the normal fre-

quency of nerve impulses discharged from the ganglion cells in voluntary contraction must lie between 300 and 5000 per second.

The complete paper will appear in the *American Journal of Physiology*, January, 1917.

A PHYSIOLOGICAL STUDY OF NOCTILUCA, WITH SPECIAL REFERENCE TO LIGHT PRODUCTION, ANAESTHESIA AND SPECIFIC GRAVITY

By Ethel Browne Harvey

CORNELL UNIVERSITY MEDICAL SCHOOL, NEW YORK CITY, AND DEPARTMENT OF MARINE BIOLOGY, CARNEGIE INSTITUTION OF WASHINGTON

Communicated by A. G. Mayer, December 5, 1916

The specific gravity of *Noctiluca* is less than that of sea water, so that normally the animals float at the surface. They contain no air bubbles or large oil drops, and their lower specific gravity must therefore be due to a lower salt content than the sea water. In more concentrated sea water, the animals shrink and in more dilute sea water they swell; the plasma membrane therefore has the usual semipermeability toward the balanced salts of sea water, i.e., permeability to water and impermeability to salts. When placed in a mixture of 4 sea water; 6 fresh water, the animals sink, their salt content being now greater than that of the surrounding medium, but later they rise to the surface, a process independent of the movement of the tentacle. They must therefore have absorbed water not only until their salt content is the same as that of the surrounding medium (when they should still sink), but enough water to make their salt content again less than the 4 s.w.: 6 f.w. mixture, thus re-establishing their normal relation to the surrounding medium. This water must be absorbed against the osmotic pressure of the salts of sea water, a process contrary to physical laws. The animals can not only lessen their specific gravity, but they can also increase it, as is shown by the fact that on windy days they sink far beneath the surface of the sea. Anesthetics, acids and alkalies, KCN, and the pure solutions of the salts of sea water do not interfere with this regulatory mechanism except when they cause irreversible changes and death of the cells; dead *Noctiluca* always sinks to the bottom.

Light production in *Noctiluca* normally occurs only on stimulation of any kind, and is a momentary bright flash; dying animals produce a bright steady glow. The luminescence is traceable to points of light coming from granules in the protoplasm. No substances were found which would cause a rhythmic flashing, comparable to the rhythmic

twitching of a muscle in pure NaCl; the only responses given were the momentary bright flashing and the steady glow. The latter response was called forth by anesthetics, cold (5°–0°C.), heat (43°–49°C.), acids and alkalies, fresh water, and a constant galvanic or interrupted induced electrical current. Cells injured by puncturing with a needle or by passage of an induced current respond to mechanical or electrical stimulation by a flash just as normal cells do. If, however, the cells are completely broken to fragments by pressing them through cheesecloth, they do not flash on stimulation although they do give a steady glow. Light production does not occur in absence of oxygen. The supposition that the cells are ordinarily impermeable to oxygen but become permeable on stimulation and on death, is not true, however, for the cells deprived of oxygen immediately give light when oxygen is readmitted. without being stimulated mechanically or in any other way.

Noctilucas may be anesthetized by certain concentrations of ether, chloroform, thymol, chloretone, ethyl and butyl alcohol so that they fail to give a flash on stimulation, but they always give a very faint glow; this disappears and the normal response returns on removing the anesthetic. *Noctilucas* differ in this respect from luminous bacteria whose light giving power can be completely anesthetized. (E. N. Harvey, *Biol. Bull.*, **29**, 1915, p. 308). Although the anesthesia of many processes, e.g., the contraction of heart muscle, cell division and growth has been shown to be independent of the consumption of oxygen, we should expect the anaesthesia of light production to be dependent on the consumption of oxygen since this process is a luminescent oxidation which will take place in solutions in a test tube free from cells. That this oxidation is different from that in most cells is shown by the fact that KCN has no effect on the light production in *Noctiluca* in relatively high concentrations, whereas it quickly paralyses the oxidations of most cells. The question arises whether the anesthesia of luminous cells is due to the fact that oxygen cannot pass through the membrane, or to the fact that it cannot be used. The latter alternative is suggested by the experiment of removing oxygen from the cells and readmitting it, thus showing that oxygen can pass the membrane at any time. The question is answered by an experiment in which the cell substance of anesthetized cells was permitted to come in contact with dissolved oxygen. Narcotized cells were broken up by shaking with sand, and it was found that they produced only a faint light whereas normal cells so treated became very brilliant. The anesthetic must therefore attack the mechanism of the utilization of oxygen in the cell, and not the permeability of the cell membrane for oxygen.

PHYSIOGRAPHIC SUBDIVISION OF THE UNITED STATES

By Nevin M. Fenneman

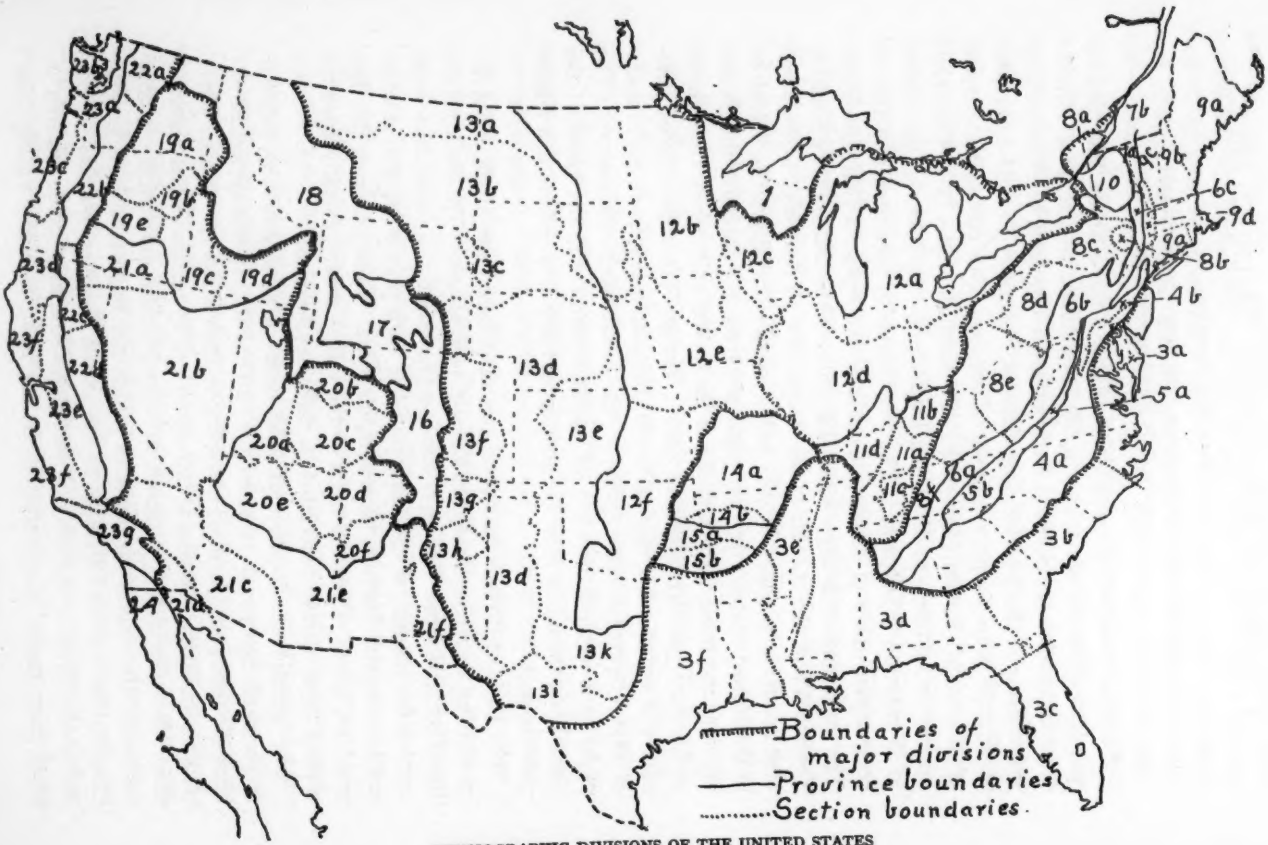
DEPARTMENT OF GEOLOGY, UNIVERSITY OF CINCINNATI

Communicated by W. M. Davis, November 24, 1916

Various attempts at subdivision of the United States into physiographic provinces have been made, beginning with that of Powell.[1] The Association of American Geographers, recognizing the fundamental importance of this problem, appointed a committee in 1915 to prepare a suitable map of physiographic divisions. The committee consists of Messrs. M. R. Campbell and F. E. Matthes of the U. S. Geological Survey and Professors Eliot Blackwelder, D. W. Johnson, and Nevin M. Fenneman (chairman). The map herewith presented and the accompanying table of divisions constitute the report of that committee. The same map on a larger scale (120 miles to the inch) will be found in Volume VI of the *Annals of the Association of American Geographers*, accompanying a paper by the writer on the Physiographic Divisions of the United States. In that paper are given the nature of the boundary lines and those characteristics of the several units which are believed to justify their recognition as such. Though the above-named committee is not directly responsible for the statements there made, many of them represent the results of the committee's conferences. The paper as a whole is believed to represent fairly well the views of the committee, though in form the greater part of it is a revision of a former publication.[2]

The basis of division shown on this map, here reproduced, is physiographic or, as might be said in Europe, *morphologic*. The divisions are based on land forms, not on climate or vegetation. If subdivision were carried far enough on the same principle each unit of the lowest order would comprise but one physiographic type. In most cases this has not been done. Even the units of the lowest order generally embrace several types closely associated in their development.

The genetic classification of land forms is now generally familiar to geographers, even to those who do not use it. In this system physiographic forms are classified according to their histories. Forms which result from similar histories are characterized by certain similar features, and differences in history result in corresponding differences of form. Generally the distinctive features which are important in a genetic classification are also obvious to the casual observer, but this is not universal. Thus a maturely dissected plateau may grade without a break from rugged mountains on the one hand to mildly rolling farm



PHYSIOGRAPHIC DIVISIONS OF THE UNITED STATES

lands on the other. So also, forms which are not classified together may be superficially similar; for example, a young coastal plain and a peneplain. Hence this map, which distinguishes physiographic types based on a genetic classification, does not in all cases make the distinctions which are most obvious to the casual observer. On the whole however, this discrepancy is not great. A very large proportion of all the boundaries shown on this map are familiar features. To have based the divisions purely on superficial features in proportion to their magnitude, would not have resulted in the making of units suited to scientific treatment.

Important physiographic differences between adjacent areas are, in a large proportion of cases, due to differences in the nature or structure of the underlying rocks. Where this is the case the two areas are distinguished on the geologic as well as the physiographic map. Distinctions based on geologic age also correspond to physiographic distinctions where the forms are so recent as to be in their first erosion cycle, as is generally the case with sheets of glacial drift. When these facts are remembered, it is not surprising that a large proportion of the boundary lines shown on the accompanying map are also geologic lines.

The segments here presented are of three orders, called respectively major divisions, provinces and sections. The basis of distinction among coördinate units is very much the same in all the orders. On the whole it may be said that contrasts in structure are stronger and more general between adjacent major divisions than between adjacent divisions of lower orders. Naturally also, the degree of topographic homogeneity is greatest in the units of the lowest order, but the reasons for calling one area a major division and another a province or a section are not clearly defined.

The degree of homogeneity in the several divisions of the same order is not in all cases the same. Homogeneity is strong in the Dissected Till Plains (12-e) which are practically everywhere submaturely dissected plains of moderate relief; also in the Snake River Plain (19–d) which is everywhere a young lava plain. On the other hand, the East Gulf Coastal Plain (3–d) is a heterogeneous area, for it grades from a young marine plain with undeveloped drainage near the coast to a maturely dissected, belted coastal plain farther inland; this case illustrates the inclusion of several types in one section by gradation, where no good dividing line is known, and where practical convenience requires that the types be considered in their mutual relations. Again, the Nevada Basin (21–b) comprises isolated mountain ranges (probably dissected block mountains) separated by aggraded desert plains; here

is an intimate intermixture of several types which are, however, so related genetically that both are accounted for by the same history. Indeed, while this work has no direct reference to teaching, there is something almost final about the requirement that a province or section should be a suitable unit for scientific treatment. This is quite as necessary from the standpoint of government surveys as from that of the university.

The committee distinctly disclaims finality for this work. With further investigation and more exact mapping some of the boundary lines here given may be shifted. The lines on this map were located by aid of the largest scale topographic and geologic maps available. Parts of the country are, however, imperfectly mapped, hence, with respect to exact plotting, the values of the several lines are unequal. All are necessarily generalized. As the result of future studies it may well be that the rank assigned to some of the units will be changed. Units of still lower orders will of course be made. Above all, the presentation of this map is not intended to preclude the use of other kinds of physical divisions like those of Supan, De Martonne, Herbertson, or Dryer. It is believed, however, that for a map of physiographic divisions, the main features of the one here presented will not be greatly changed.

The uses of such a map are of two general classes, (1) for scientific (explanatory) description, and (2) for comparative studies with other geographic elements. In the former, physical features are looked upon as the product of geologic processes, in the latter they constitute factors or conditions of life and human activity. In the former aspect they are an effect; in the latter a cause. The potency of such causes can only be known when statistics of population, agriculture and industry and even politics are graphically shown with due respect to natural units. It is plain that if matters statistical are to be represented on a map of natural divisions, and things human are to be discussed in terms of their physical setting, the value of the relations discovered will depend largely on the character of the natural divisions and their proper delimitation.

In the following table the names of major divisions are printed in italics; the provinces are numbered, and the sections lettered. The province number and the section letter correspond to those on the map.

Table of physiographic divisions of the United States

Laurentian Upland.—

1. Superior Upland

Atlantic Plain.—

2. Continental Shelf
3. Coastal Plain: (a) Embayed section; (b) Sea Island section; (c) Floridian section; (d) East Gulf Coastal Plain; (e) Mississippi Alluvial Plain; (f) West Gulf Coastal Plain.

Appalachian Highlands.—

4. Piedmont province; (a) Piedmont Upland; (b) Triassic Lowland.
5. Blue Ridge province: (a) Northern section; (b) Southern section.
6. Appalachian Valley province: (a) Tennessee section; (b) Middle section; (c) Hudson section.
7. St. Lawrence Valley: (a) Champlain Valley; (b) Northern section.
8. Appalachian Plateaus: (a) Mohawk section; (b) Catskill section; (c) Allegheny Plateau (glaciated); (d) Allegheny Plateau (Conemaugh section); (e) Kanawha section;* (f) Cumberland section.
9. New England province: (a) New England Upland; (b) White Mountain section; (c) Green Mountain section; (d) Taconic section.
10. Adirondack province.

Interior Plains.—

11. Interior Low Plateau:† (a) Highland Rim Plateau; (b) Lexington Plain; (c) Nashville Basin; (d) not named.
12. Central Lowland: (a) Eastern Lake section; (b) Western Lake section; (c) Wisconsin Driftless section; (d) Till Plains; (e) Dissected Till Plains; (f) Osage Plains.
13. Great Plains: (a) Missouri Plateau (glaciated); (b) Missouri Plateau (unglaciated); (c) Black Hills; (d) High Plains; (e) Plains Border; (f) Colorado Piedmont; (g) Raton section; (h) Pecos Valley; (i) Edwards Plateau; (k) Texas Hill section.

Interior Highlands.—‡

14. Ozark province: (a) Salem-Springfield plateaus; (b) Boston Mountains (plateau);
15. Ouachita province; (a) Arkansas Valley section; (b) Ouachita Mountains.

Rocky Mountain System.—

16. Southern Rocky Mountains (to be divided into sections).
17. Wyoming Basin.
18. Northern Rocky Mountains (to be divided into sections).

Intermontane Plateaus.—

19. Columbia Plateau: (a) Walla Walla Plateau; (b) Blue Mountains; (c) Payette section; (d) Snake River Plain; (e) Harney section.

* Likewise part of the Allegheny Plateau.

† In the report of the committee this is called the Highland Rim Province. Messrs. Campbell and Matthes do not concur in the exclusion of this entire province from the Appalachian Highlands. They would divide it, and assign the eastern half to the Appalachians.

‡ In the report of the committee this is called the Ozarkian Highlands.

20. Colorado Plateaus: (a) High Plateaus of Utah; (b) Uinta Basin; (c) Canyon Lands; (d) Navajo section; (e) Grand Canyon section; (f) Datil section.
21. Basin-and-range province: (a) Oregon lake section; (b) Nevada Basin; (c) Sonoran Desert; (d) Salton Trough; (e) Mexican Highland; (f) Sacramento section.

Pacific Mountain System.—

22. Sierra-Cascade Mountains: (a) Northern Cascade Mountains; (b) Middle Cascade Mountains; (c) Southern Cascade Mountains; (d) Sierra Nevada.
23. Pacific Border province: (a) Puget Trough; (b) Olympic Mountains; (c) Oregon Coast Range; (d) Klamath Mountains; (e) California Trough; (f) California Coast Ranges; (g) Los Angeles Ranges.
24. Lower Californian province.

[1] An excellent account of these attempts has been given by Joerg, W. L. G., *Assoc. Amer. Geogr., Annals*, 4, 1914 (55–84), 22 maps.

[2] Fenneman, N. M., *Assoc. Amer. Geogr., Annals*, 4, 1914 (84–134), 3 maps.

ON THE COMPOSITION OF THE MEDUSA, CASSIOPEA XAMACHANA AND THE CHANGES IN IT AFTER STARVATION

By S. Hatai

TORTUGAS LABORATORY, CARNEGIE INSTITUTION OF WASHINGTON

Communicated by A. G. Mayer, December 13, 1916

Cassiopea may be divided into three distinct parts; mouth-organs, umbrella and velar margin. Since these three parts differ not only morphologically, but also in their absolute weights, as well as in the relative amount of cellular and non-cellular constituents, it was thought desirable to study the normal growth of these parts in order to determine whether the starving Cassiopea loses weight uniformally or whether the loss is dissimilar in the three parts concerned. A large number of observations were also made on the undivided Cassiopeas.

The observations made on the normal Cassiopea may be summarized as follows: (1) Relative weights of mouth-organs, umbrella and velar margin differ somewhat according to the size of the entire body. (2) The water content of the entire body, as well as of different parts, is practically identical throughout the animal's life cycle, so far as followed. (3) The percentage of nitrogen in the solids is highest in the smallest medusa, and the values decrease progressively with increasing body weight. The percentage of nitrogen is highest in the velar margin and decreases in the mouth-organs and umbrella in the order named.

All these observations are interpreted as indicating that in the structures in which the cellular elements are abundant, the nitrogen content tends to be high. (4) The ash content, like the water content, is probably the same not only in the Cassiopea as a whole, but also in each of the three parts throughout the life cycle. The next step was to determine the changes in Cassiopea as the result of starvation.

Mayer ('14) reported (*Carnegie Inst. Publ.*, No. 183, pp. 55–84) that in Cassiopea the percentage of nitrogen to the total solids remains constant during the entire period of starvation. Mayer infers from this that "no appreciable chemical change occurs in the composition of its body, and that there is no appreciable selective use of different substances at different times during the progress of starvation." This is remarkable since the starving mammalian body reveals a totally different relation owing to the rapid disappearance of reserve substances such as carbohydrates and fats during the earlier period of starvation, followed by a slow consumption of protein substances later. Thus the starving mammalian body gives different percentage values for the nitrogen at different periods of starvation, especially in the earlier stages.

At present my observations are limited to the composition of the Cassiopeas at the end of the starvation experiment, while I reserve the question of progressive changes during the stages of inanition for the future.

For this purpose eight freshly caught normal Cassiopeas, having different body weights, were subjected to starvation. The starvation was accomplished by placing the animal in filtered sea water. The filtration was made with all the precautions adopted by Mayer ('14) and the water (4500 cc.) in the vessel was changed once every day. Briefly summarized the results of the observations made on the starved Cassiopea are as follows:

1. In general the smaller Cassiopea loses relatively more in weight than does the larger Cassiopea.

2. The percentage of water found in the entire body, as well as in the three different parts is nearly the same in all sizes of Cassiopea. However the values of water content in the starved, appears to be slightly higher than that found in the normal Cassiopea.

3. The nitrogen content of the entire body is higher in the smaller than in the larger Cassiopea, as in the case of the normal animals.

4. However the absolute amount of nitrogen found in the starved Cassiopea is considerably higher than in the normal having the same body weight. It was noted also that although high when compared with the normal, equal in weight to the starved animal, it is very low for the initial body weight of

the starved animal. This shows that the nitrogen also has been consumed during the period of starvation.

5. The nitrogen contents for the different parts of the body are similar in their relations to those found in the normal Cassiopea.

6. The loss in weight of the different parts is of such a character that their proportions in the starved remain similar to those in the normal Cassiopea.

From his data Mayer concluded that the percentage of nitrogen in the solids is independent of the period of starvation, and is practically identical with that obtained from the non-starved Cassiopea. I have however found, as stated above, that starvation tends to increase not only the percentage of nitrogen in the solids, but also that the absolute amount of nitrogen shows an increase when the starved Cassiopeas were compared with the normals having the same body weight.

The discrepancy between the conclusions drawn by Mayer and by myself is I believe due to the fact that Mayer's observations were limited to the larger Cassiopeas (body weights over 100 grams) in which the percentage of nitrogen in the solids shows little variation following the large variations of the body weight, while the variations in the nitrogen are quite noticeable in the Cassiopeas of smaller size. I may add here that the data given by Mayer in his table 2, show also a slight indication of a difference in the nitrogen content between the normal and starved Cassiopeas.

I have applied Mayer's law for the loss of weight in starving Cassiopeas to my own data, and found a satisfactory agreement between the observed and calculated values by the formula

$$Y = 83.58\ (1\text{–}0.05)^{x-1}$$

where Y represents body weight and X the number of days of starvation. My formula differs from that of Mayer in that I give the exponent as $(x-1)$ while Mayer gives it simply as x. He did not however consider the loss during the first day as being due to starvation, for during this time considerable quantities of undigested food and slime are discharged and the loss is thus excessive and irregular. Hence our formulas are in essential accord one with the other, both applying *after* the medusa has discharged its undigested food and its gastric cavity is empty. The lower body weight obtained, as compared with that calculated at an earlier period of starvation, was probably due to the frequent handling of the animal in order to determine the body weight daily.

A more detailed paper will appear in a volume of *Researches* from the Department of Marine Biology of the Carnegie Institution now in preparation.

STUDIES OF THE MAGNITUDES IN STAR CLUSTERS, IV. ON THE COLOR OF STARS IN THE GALACTIC CLOUDS SURROUNDING MESSIER 11

By Harlow Shapley

MOUNT WILSON SOLAR OBSERVATORY, CARNEGIE INSTITUTION OF WASHINGTON

Communicated by G. E. Hale, December 18, 1916

The investigation of phenomena relating to brightness and color in the clusters that are located in rich regions of the sky necessitates a knowledge of certain properties of the neighboring stars. A catalog of the members of any cluster must inevitably include some stars extraneous to the group, and to a greater or less extent the colors and magnitudes of such objects will influence the interpretation of the results. For the globular clusters, particularly in high galactic latitude, the preponderance of cluster stars over those that chance to be included with them is such that no serious effect is to be feared. For the open clusters, however, and especially for those in the Milky Way, the number of background or foreground stars may easily exceed the number of physical members of the cluster, even at the center of the group. In these cases it becomes necessary to make a direct and special study of the extra-cluster stars in fields far enough removed from the cluster to be practically free of its outlying members, but not so distant that the field stars can not be considered representative of those that in projection appear commingled with the cluster.

A study of the colors in the open galactic system Messier 11 (N. G. C. 6705) has been completed with the compilation of a catalog of the magnitudes and colors of nearly five hundred stars. The cluster is situated in the constellation Scutum Sobieski, in one of the densest of the great star clouds of the Milky Way. Unlike the stars in Messier 67, in which all the fainter objects are yellow and red (and similar in most respects to the neighboring non-cluster stars), those in Messier 11 fall into all color classes, but with a distinct preference for the bluer types. The group is but four degrees from the mid-line of the Galaxy; Messier 67 is in galactic latitude $+34°$. To find whether the presence of blue faint stars in Messier 11 is a peculiarity of the cluster or is only a result of its low galactic latitude and its association with the star clouds, a special study in several neighboring fields was undertaken. This investigation bears directly on the relation of the cluster to the surrounding stellar masses, and also yields information regarding the nature of the galactic clouds themselves and the spectra and intrinsic luminosities of the numberless stars that compose them.

Nearly forty plates, made with the 60-inch reflector, have been used in the present work. In addition to the above-mentioned catalog of stars in and near the cluster, photographic and photovisual magnitudes of more than 300 stars in four other neighboring fields have been determined by comparisons with the cluster and checked by direct comparisons with the Polar Standards. Special care was taken to avoid

PERCENTAGE FREQUENCY OF COLORS IN GALACTIC CLOUD FIELDS

	COLOR CLASS											
	b0	*b5*	*a0*	*a5*	*f0*	*f5*	*g0*	*g5*	*k0*	*k5*	*m0*	*m5*
Field I	0	2	6	12	18	12	12	16	12	4	2	4
Field II	1	3	7	17	13	8	8	8	12	12	5	6*
Field III	0	2	17	29	10	10	10	8	4	8	4	0
Field IV	0	0	2	27	24	22	10	0	8	4	4	0
All fields	0.3	2	8	20	15	11	9	8	10	8	4	4†

* 2.5% have color indices greater than +2.00.

† 1.3% have color indices greater than +2.00.

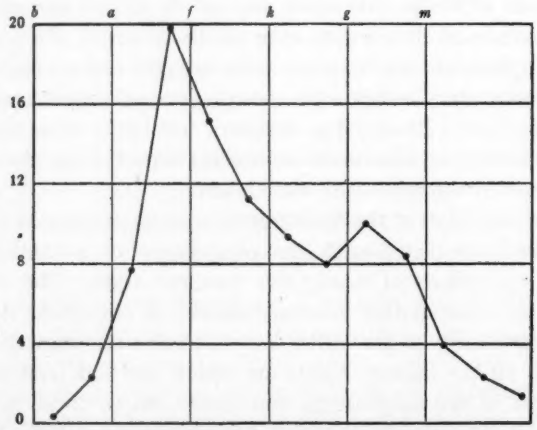


FIG. 1. FREQUENCY OF COLORS IN GALACTIC CLOUDS. ABSCISSAE ARE COLOR CLASSES; ORDINATES ARE PERCENTAGES.

the use of questionable photographs or of doubtful star images, and systematic errors of any importance are probably completely absent from the results. The average probable error of a color index varies in the different fields from a tenth to a twentieth of a magnitude.

The interval of photovisual magnitude investigated is from 11.0 to 15.5. The relative frequency of color class is given in the accompanying table for each field separately and for the four together. Figure 1

illustrates the combined results. The most important features are the great diversity of color index and the general resemblance of this frequency curve to that for the brighter stars in the immediate neighborhood of the sun.[1]

The full presentation of the material will appear in two forthcoming *Contributions from the Mount Wilson Solar Observatory*. Some of the conclusions to be drawn from the results may be summarized as follows:

1. Messier 11, in common with many other open clusters, does not differ greatly in the brightness and color of its stars from the non-condensed fields which surround it. Though its members without doubt form a distinct physical system, it is probably at approximately the same distance from the sun as the stars of corresponding color and apparent magnitude in the galactic cloud. In fact, the study of colors and magnitudes tends to confirm Barnard's inference, based on general appearances, that the cluster may be nothing more than a nucleus of the extensive surrounding stellar masses.[2] In this similarity to its environment, therefore, Messier 11 is vastly different from a globular cluster, such as Messier 13, which is apparently much more distant than the non-cluster stars in the same part of the sky.[3]

2. The distance of all the stars of Messier 11 is sensibly the same; hence apparent magnitudes represent relative absolute luminosities. Plotting the average color index against magnitude, as in figure 2, a striking progression of color with decreasing brightness is revealed. The neighboring stars, though showing much the same frequency of color class, are probably more scattered in space, and each interval of magnitude includes stars varying greatly in distance and of most diverse spectral types. The indefinite relation between magnitude and color index here observed is, therefore, not unexpected.

3. Accepting the present results as dependable and taking color class and spectral type as closely analogous, the presence of the small and negative color indices in the galactic clouds indicates either that the stars are at a great distance or that they are not similar in luminosity to the brighter stars near the sun. In the light of recent researches on the dispersion of absolute magnitudes of B and A-type stars, the first alternative is decidedly preferable. The parallax of a typical B-type star of apparent magnitude 13.5 would be about 0".0002, a value that may be taken as a first estimate of the distance of Messier 11. The *b*-class stars in the surrounding clouds are between magnitudes 13 and 15 and some are possibly fainter—suggesting still greater distances for parts of the stellar background.

4. Just as a wide range of color class and the presence of negative

color indices in the globular cluster Messier 13 indicated the absence of scattering of light in space,[4] the similar properties of the faint stars in Messier 11 and the galactic clouds show that in this direction also light is not appreciably diminished by scattering.

5. The present investigation contributes to the problem of the color of the faint stars, and consequently, if space absorption is accepted as ineffective, to the question of the extent and character of the galactic system. The increase of the minimum color index with decreasing brightness has been observed by Seares[5] for the north polar stars, galactic latitude +28°, by Hertzsprung[6] and Seares[7] in the open cluster

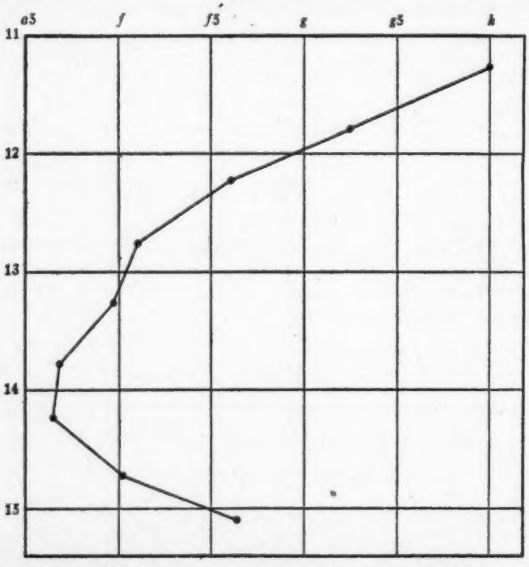


FIG. 2. RELATION BETWEEN COLOR AND LUMINOSITY IN MESSIER 11. ABSCISSAE ARE COLOR CLASSES; ORDINATES ARE APPARENT MAGNITUDES.

N. G. C. 1647, galactic latitude −15°, by the present writer in and around Messier 67,[8] galactic latitude +34°, and in the neighborhood of Messier 13, galactic latitude +40°. In none of these fields have very small or negative color indices appeared among the fainter stars;[9] and a definite, accessible limit to the galactic system was thus suggested, at least for the higher galactic latitudes. The first divergence from this tendency was found in the field of the variable star XX Cygni,[10] galactic latitude +13°, where 13th magnitude stars of color *b* were found, as well as a number of faint *a*'s. Similarly, faint blue stars have been recently observed by the writer in the region of the Perseus cluster,

galactic latitude $-3°$. With the addition of the data for Messier 11 and for the four neighboring fields, it appears that the redness of the faint stars will be found to depend, as might be expected, upon galactic latitude, and in the mid-galactic regions will vary with the density of the star clouds.

6. The presence of negative color indices for faint stars in the three widely separated galactic regions mentioned above shows that, if these stars are typical in absolute brightness, the dimensions of the galactic system in the plane of the Milky Way are many times greater than has been inferred from studies of variables and investigations of the motions and magnitudes of the brighter stars.

[1] For instance, see Parkhurst, Yerkes Actinometry, *Astroph. J., Chicago, Ill.*, **36**, 1912, (218, 225) and figure 5 on p. 56 of *Mt. Wilson Contrib.*, No. 116.

[2] *Berkeley, Univ. Cal. Pub., Lick Obs.* **11**, 1913, Plate 62.

[3] *Mt. Wilson Contrib.*, No. 116, 1915, (81 ff.).

[4] These PROCEEDINGS, **2**, 1916, (12–15).

[5] *Astroph. J., Chicago, Ill.*, **39**, 1914, (361–369); [*Mt. Wilson Contrib.*, No. 81].

[6] *Ibid.*, **42**, 1915, (92–119); [*Mt. Wilson Contrib.*, No. 100].

[7] *Ibid.*, **42**, 1915, (120–132); [*Mt. Wilson Contrib.*, No. 102].

[8] *Mt. Wilson Contrib.*, No. 117, 1916. The unpublished results for the fields near Messier 13 are provisional.

[9] In N. G. C. 1647 Hertzsprung (*loc. cit.*) finds one star of photographic magnitude 12.40 that is apparently of spectral type B.

[10] *Astroph. J., Chicago, Ill.*, **42**, 1915 (148–162); [*Mt. Wilson Contrib.*, No. 104].

THE COLOR OF THE STANDARD POLAR STARS DETERMINED BY THE METHOD OF EXPOSURE-RATIOS

By Frederick H. Seares

MOUNT WILSON SOLAR OBSERVATORY, CARNEGIE INSTITUTION OF WASHINGTON

Communicated by G. E. Hale, December 18, 1916

An earlier note in these PROCEEDINGS describes a method of measuring the color of a star which depends upon the ratio of the exposure times necessary for its blue and yellow light to produce images of the same size.[1] A comparison of the observed exposure-ratio with a curve derived by combining similar ratios for stars of known color affords a means of expressing the results in terms of color-index or color-class.

The method is expeditious, and, under favorable conditions, precise; it is entirely independent of stellar magnitudes, and hence avoids the systematic errors which so easily enter as a result of uncertainties in the magnitude scales or in their zero points. Moreover, the method is direct, in the sense that color is measured and not inferred from observations of spectral type. The results thus include that part of the color

which is a function of the star's intrinsic luminosity and also the influence of a possible scattering of light in its passage through space.

The following paragraphs indicate briefly the results found with the 60-inch reflector for about 80 of the North Polar Standards between magnitudes 2.5 and 16.3 whose colors had already been derived by a comparison of their photographic and photovisual magnitudes. The brighter stars, owing to their distribution, had to be observed separately. To avoid photographic difficulties, the plates for these were all of the same emulsion. Further, all the brighter objects were reduced to an equivalent of the 11th magnitude, approximately, by means of screens and diaphragms and then given the following series of exposures on a Cramer Inst. Isochromatic plate:

a. Yellow images (through yellow filter), 16^s, 32^s.
b. Blue images (without filter), 2^s, 4^s, 8^s, 16^s, 32^s.
c. Yellow images (through yellow filter), 16^s, 32^s.

In a few cases the exposures for the yellow images were 32^s and 64^s.

The data on each plate were reduced graphically and gave two values for the exposure-ratio corresponding to equal blue and yellow images. In general, three plates, exposed upon different nights, were used for each star. To derive a reduction curve connecting color-index with exposure-ratio, the mean ratios were collected in groups according to color-index. Since each group contains stars of widely differing magnitudes, the resulting curve is probably free from the influence of any systematic error in the color-indices depending on brightness, and should give a reliable value for the color of any star whose exposure-ratio has been determined under conditions similar to those underlying the derivation of the curve itself. Moreover, the question of systematic errors in the original color-indices, which were taken from the Mount Wilson investigation of the photographic and photovisual magnitudes of the polar stars, can be put to a direct test.

For this purpose the color-index of each star was determined from its exposure-ratio by means of the reduction curve and compared with the original value based on magnitudes. The differences, arranged according to the brightness of the stars, should reveal any systematic error in the original system of color values, and thus test the accuracy, relative to each other, of the photographic and photovisual scales of the North Polar Standards. The means of these differences for groups of stars of a limited range of brightness are given in the second column of the table. Obviously there are no systematic differences of any importance which depend upon magnitude.

In order that the investigation might be extended to the fainter stars,

five additional photographs were made with single yellow exposures of 128^s and 10^m, respectively, and corresponding blue exposures of 8^s, 16^s, 32^s, 64^s, and 1^m, 2^m, 4^m. For these the full aperture of 60 inches was used, and the measurable images of all the stars shown were included in the discussion. On the plates of the first series described above, the images for objects of all degrees of brightness were approximately of the same size; but here there was no attempt at equalization.

The question as to the effect of size of image upon the exposure-ratio had accordingly to be examined. Inasmuch as the gradation of a photographic plate is to some extent a function of the wave-length of the light producing the image, the necessity for some correction of the exposure-ratios derived from these plates of longer exposure was anticipated. Special photographs of bright stars taken with a wide range in aperture, but with the same series of exposure times, gave data for a provisional determination of the corrections. These seem to be moderate, unless the images used for finding the exposure-ratio are very large. The corrected mean differences are in the third and fourth columns of the table. The two plates of 10^m exposure were of another emulsion, requiring a different reduction curve, and were accordingly reduced to the standard curve which applies to the remainder of the data.

Finally, an additional group of four plates of the original emulsion was exposed in November, four months after the others had been taken. The results for these are in the fifth column of the table. As the small correction depending on the size of the image is not yet well determined, it has been omitted in the case of this last group of plates. It will be noted that the results are substantially as before.

DIFFERENCE BETWEEN COLOR-INDEX FROM MAGNITUDES AND THAT FROM EXPOSURE-RATIO

(Unit = 0.01 mag.; number of values in parentheses)

PHOTOGRAPHIC MAGNITUDES	DURATION YELLOW EXPOSURES				WEIGHTED MEAN DIFFERENCE	NUMBER OF STARS IN GROUP
	$16, 32^s$	128^s	10^m	16^s to 8.5^m		
3.5	0(12)				0	2
5.7	+3(16)				+3	3
6.5	−2(20)				−2	4
7.5	+2(20)				+2	3
8.6	−3(24)				−3	3
9.4	−7(18)				−7	3
10.6	−4(48)				−4	6
11.4	+5(54)	+5(24)	+4(14)	−5(23)	+3	8
12.3	+2(4)	+3(15)		−5(15)	−1	7
13.3		+6(25)	+1(17)	+4(25)	+4	12
14.1		0(5)	+3(26)	−5(7)	+1	13
14.8			0(26)		0	13
15.5			−6(21)	+7(6)	−3	11

The last two columns of the table give the weighted mean differences between the Mount Wilson color-indices and the color-values derived from the exposure-ratios, and the number of stars included in each group.

The close agreement in the two series of results shows clearly enough the usefulness of the exposure-ratio method, and indicates that the photographic and photovisual magnitude scales for the North Polar Standards are substantially in the proper relation to each other. The agreement affords no test of the presence of errors affecting the two scales equally; but there can be no important divergence of either scale relative to the other, for the differences in the color-values derived by the two methods would then show a progressive change with magnitude.

In general, the precision of the method of exposure-ratios is excellent. For example, the probable error of a color-index derived from a single exposure-ratio is about 0.07 magnitudes. This value is based upon the results for the 36 stars for which there are five or more separate determinations, and speaks well for the uniformity of different plates of the same emulsion. Puzzling abnormalities have occasionally occurred, but the cases thus far met with are perhaps to be attributed to causes external to the plates themselves. Each emulsion will undoubtedly require a special investigation for the determination of factors which will reduce it to the standard curve connecting exposure-ratio and color-index, but this should not be a matter of any great difficulty. Nevertheless, the limitations of the method can not be fixed until a number of questions of this sort have been examined.

In the meantime, however, it may be noted that the colors of the Polar Standards, brighter than the 13th magnitude, have been determined to about the same precision as was reached in the investigation of the magnitude scales, with an expenditure of time and labor which was perhaps a tenth of that required for the earlier investigation. All of the observational data for the present investigation, including about 80 photographs and nearly 400 separate determinations of color, were obtained during three nights with a total of less than 22 hours observing time.

One of the most interesting consequences of this recent determination of the colors of stars near the Pole is to be found in the confirmation it affords of a result previously announced,[2] namely, that there are no faint stars in this region with negative or small positive color-indices. The lower limit of the color-index gradually increases as the fainter stars are approached, and at the 16th photographic magnitude its value is of the order of +0.5 magnitude. Although this state of affairs was

indicated with some certainty by the comparison of photographic and photovisual magnitudes, it is of interest to find it appearing as the result of an entirely different method of investigation.

The absence of faint white stars is known to be a characteristic of other regions as well,[3] but it must not be inferred that such objects are not to be found anywhere in the sky. In Selected Area No. 88, for example, in one of the outlying clouds of the Milky Way, photographs by the exposure-ratio method indicate that the stars of the 14th or 15th magnitude are nearly normal in color and thus include a considerable number of objects that are white. Mr. Shapley has also accumulated evidence of this sort in connection with his study of clusters.

[1] These PROCEEDINGS 2, 1916, (521–525).

[2] *Astroph. J., Chicago, Ill.*, 39, 1914, (361–369); [*Mt. Wilson Contrib.*, No. 81].

[3] *Ibid.*, 40, 1914, (187–204), 42, 1915, (92–119), (120–132); [*Mt. Wilson Contrib.*, Nos. 83, 100, 102].

TERRACING OF BAJADA BELTS

By Charles Keyes

Communicated by W. B. Clark, December 16, 1916

For the local stream-trenching and the resultant terracing of the higher zones of those long uniform slopes which so often spread out from the foot of desert mountain ranges there is an explanation very much simpler than any of the numerous ones yet offered. It has the advantage of being in strict accord with the regular and ordinary phases of erosional action which recent critical observation shows to be now at work as vigorously and as effectively as they have been in any past period. It is, in effect, nothing more than a reiteration, in a somewhat new form to be sure, of the old law of parsimony which forbids the unnecessary multiplication of explanatory elements and agencies.

In all late physiographical writings in which the term bajada is used it is unfortunately misconstrued. Spanish-speaking Americans do not seem ever to have given the title so broad a meaning as that sometimes attached to it. If the name is to remain a useful geographical term of description it should be allowed to retain something of its original significance, and should be restricted in its application to the steeper slopes of the desert piedmonts. Without exception bolsons appear to present four distinct physiographic areas, or belts, three of which are plains. There is the central, more or less level tract, sometimes covered for a period of a few days or weeks of each year by a

thin sheet of gathered storm-waters—the *playa*. There is next the long smooth slope of low inclination, having about a 2% gradient. This is followed by a short, steeper slope, with a 4% grade, to which the name *bajada* is properly given. Fourth is the mountainous periphery.

In the literature on the arid regions the bajada-belt is usually treated as one of the most conspicuous and important drainage features, as formed by prodigious outwash from the peripheral highlands of basins of centripetal drainage, and as consisting of a series of great coalescing delta-fans. To this interpretation several strong objections arise. Discordant facts greatly outweigh the supporting evidence. The piedmonts of western deserts most frequently described chance to be on the margins of the Great Basin where the lofty Wasatch range and the still loftier Sierra Nevada produce effects which are not at all typical of the true Basin-ranges. In other places the steeper parts of the intermont plains, or bajadas, often mark belts of resistant rocks and are almost devoid of soil. In still other cases the extension of the even-sloping bajadas up into the valleys of mountain-arroyos is manifestly the direct result of rapid and tremendous drifting of soils from the low-land plains, rather than of the gravitational flow of detrital materials from the adjacent highlands. The volume of finer rock-waste brought down from a desert-range by the infrequent storm-waters is not by any means what might be expected; it is, in reality, phenomenally small. As more fully stated elsewhere the yearly amount washed down by the rains may be swept away by the winds in a single day.

In spite of the fact, then, that the *bajada* is often a belt of thick, adobe soils, of drifting sands, or of sporadic outwash from the nearby mountains, it is also still oftener true that it is an area of the most indurated rock, so free from soil that, as W. J. McGee describes, "the horses' shoes beat on the planed granite, and schist and other hard rocks in traversing the plain 3 or 5 miles from the mountains." Then, again, there are typical bajadas widely separated from desert ranges by deep longitudinal valleys which hug the mountain bases—conditions under which there is no possible chance for the outwash from the highland to reach the plain. The Sandia, Manzano and Caballos sierras, in central New Mexico, are a few of the many notable examples.

The terracing of arroya-courses in the piedmont belt appears usually to be merely the outcome of a vigorous contest which is constantly waged between the local, temporary aggrading of wind-driven soils or sands on the one hand, and on the other hand by the weak degrading action of the infrequently running mountain torrent. The phenomena of bajada-terracing is not, as urged by some physiographers, a necessary

consequence of the general lowering of the highlands by stream-action, while the intermont lowlands are being filled up; because some of the best examples of terracing border broad plains having rock-floors. For the same reason it does not appear possible that there ever occurs during so-called topographic maturity an adjustment by water action between one bolson and an adjacent lower one which results in the terracing of the higher. There is little or no actual evidence to show that bajadas were all formed during periods of glaciation; since some of the most typical expressions of these sloping plains are found surrounding low knolls near sea-level, and far below all possible altitudes of glacial action in the region. Neither does it appear likely that bajadas were built up during interglacial epochs of materials which accumulated in the mountains when the latter were covered with ice; for this does not explain the many cases in which rock-floors are present. Nor is it any better to postulate a recent increase of temperature and a different distribution and amount of rain-fall abetted by the advancement of the area in the geographic cycle; for terracing is now going on before our very eyes at an astonishingly rapid rate, and as quickly is it also completely obliterated. In many localities over-grazing is manifestly a potent and direct cause of the tremendous recent trenching and corrading by sporadic storm-waters of the soft temporary soil accumulations in the desert.

In recently setting forth reasons for believing that the gradational effects displayed by the intermont plains of arid regions are mainly accomplished by means of the winds, I have attempted to point out the fact that the action takes place chiefly uphill instead of gravitationally down-stream as in the case of running waters. I have also endeavored to emphasize the point that the relatively steep slope of the bajada-belt represents the highest possible wind-gradients, just as the river-bed in a peneplain approaches the lowest possible water-gradient. This statement appears to be amply supported by the results arising from the artificial diversion of arroyo-courses over the smooth bajadas.

Were the leveling tendencies of the winds wholly absent from the desert regions it is quite possible that the corrasive effects of what desert waters there are would be much the same as they are in humid lands, differing only in degree. This is well shown in the cases of wing-dams that have been constructed to protect lines of railway from the disasters of the flood-sheet and the latter has come before the earth-works have had time to be leveled by the winds. In one instance in particular the culvert and track were washed out in less than an hour's time, and a canyon, 75 feet deep, 50 feet wide and several miles long,

was excavated in the smooth surface of the sloping plain. By the time a permanent bridge was built to span the deep trench the winds had filled the entire excavation, so that where a yawning chasm had been was again as smooth as the rest of the plain, and the wing-dams also had melted down into the general evenness of the desert's surface. For several years, until it was finally replaced by an earthen grade, travelers were wont to express great wonderment at the possible utility of so fine a steel bridge resting on the smooth sands of the desert plain.

The Socorro arroyo, in central New Mexico, presents another pertinent case. There is water running in the shallow wash once or twice a year, the supply coming off the lofty Magdalena peak 20 miles away. For many years this arroyo, which divides the town of the same name, has given the residents an infinite amount of trouble. That its 2% grade really produces torrential conditions when the waters do run is indicated by the fact that the arroyo-bed is composed largely of pebbles and boulders, many of the latter attaining a size of 2 feet. In order to obviate the yearly inconveniences of flooding it was determined, a few years ago, to divert the channel 4 miles above the town. This change of course was accomplished by cutting a narrow trench from the bed of the water-way through its bank to a point some 50 yards to one side, where the general plain was slightly lower than the bed of the wash at the head of the ditch. A low dam was thrown up obliquely across the arroyo by piling up boulders from the bed. The theory was that the first water coming down the wash would flow out the ditch, or spill-way, and there would soon cut a deep channel; and that eventually this would carry away all of the future flood-waters. Results more than fulfilled expectations. The first time the dry creek became a brook there was trenched in a single night a chasm 50 feet deep for a distance of more than a mile down the slope of the plain. The materials from the great artificial canyon spread out over the railroad tracks 3 miles away to a depth of 7 feet and to a width of half a mile, necessitating the rebuilding and raising of the grade for a distance of several miles. These two illustrations might be infinitely repeated.

Observations such as these demonstrate beyond a shadow of doubt that whenever around the desert ranges there are alluvial fans, or accumulations of unindurated deposits, it is possible at any time for profound and rapid dissection to take place through means of the copious but infrequent storm-waters. The recent notes of C. L. Baker, A. C. Trowbridge and others on the eastern slopes of the Sierra Nevada, amply confirm these conclusions. Since, however, the restricted areas in which the latter investigations were undertaken lie at the foot of

lofty ranges and border a region of moister climate the action of running waters is more nearly normal than in the Great Basin proper. Yet, in the interior of the latter the same phenomena are also well displayed. Around the southern rim of one remarkable desiccated Las Vegas, in southern Nevada, in the low Spring, Newberry and Eldorado ranges, the bajada is frequently dissected and terraced in a singular manner. So extensive is it that it may be clearly distinguished even at a distance of 10 or a dozen miles. Dissection and terracing are also everywhere apparent around the region of the excessively dry basins of Death valley, the Armagosa plains and the Mojave desert.

In this connection there is one circumstance which not only A. C. Trowbridge and C. L. Baker appear to have overlooked, but likewise F. L. Hess, J. E. Spurr, G. K. Gilbert, H. W. Fairbanks and other earlier writers. In the descriptions of the terrestrial deposits the origin of the latter is ascribed entirely to water-action. No account seemingly is taken of possible assistance of wind-action in piling up locally these masses of debris. Photographs of the region, which the authors named reproduce, display unmistakable signs of plenty of wind-work. On this point direct personal observation is even more conclusive. In the building up of the so-called alluvial fans and of the bajada when composed of fine materials the winds appear to be the controlling power. Arroyo-waters seem mainly to be merely modifying agents, supplying some coarse rock-waste from the mountains, but largely locally turning back the materials brought in up-grade from the lowland plains. The effects simulate the alluvial fans of humid regions; but they are not by any means their exact counterpart.

After the lower reaches of the canyons, immediately before they debouch upon the plains, become over-filled in the course of a few months or a few years, with the wind-driven sands and dusts and are eolicly aggraded they are readily dissected and even terraced by the first appearance of heavy storm-waters on the mountains. For accomplishing these results the time-element is certainly not so interminably long as has been commonly supposed. It is not necessary to stretch it back to the Glacial period, and far beyond. It is not to be gauged by tens of millenia. Its span is to be measured not even by years, but by months or weeks. It is known to have been limited by a single rising and setting of the sun.

The local dissection and terracing of the so-called alluvial fans in arid regions may have a significance still broader than that commonly ascribed to it. In the explanation of these phenomena we may have the key to the specific method by which general leveling and lowering of

desert lands are accomplished. Neither feature is confined to areas which are situated at the mouths of canyons. Both are displayed in bajada belts where rock-floors are present and where the once even surfaces are worn out on the beveled edges of tilted strata. The Calico range, in the Mojave desert, north of Daggett, California, is a notable but not an isolated example. No eminence of the Great Basin region appears, at first glance, to be more certainly a 'lost mountain,' a lofty range buried up to its shoulders in its own debris. The bajadas on either side of the ridge all but meet over its summit. So low and rounded is the crest that manifestly there is no opportunity for extensive outwash around the borders. There is, in this instance, not only remarkable dissection of the bajada belt taking place at the present time but a widening of the apparent lines of drainage into wide flat-bottomed esplanades with deep reentrants. Elsewhere there is the anomaly of a long sinuous terrace several hundreds of feet in height separating the higher general plains-surface from the lower local plains-level. In this we get a glimpse of the formation of those heretofore inexplicable but characteristic desert features known as plateau-plains. In their last stage the isolated Tówa-yal-lané, Acoma and Chupadera mesas, of New Mexico, are conspicuous illustrations. To this phase of the problem attention is later turned.

The feature of desert bajada-terracing, when explained upon a strictly aqueous basis, cannot but lead to complete misinterpretation. The phenomenon has no necessary connection with former and greater stream-activity. It is one of the wide-spread characteristics of desert lands. It is far more largely the result of wind-action than of water-action. Its marvellous aspect is the great rapidity with which it takes form. It is, in reality, one of the subordinate expressions of regional eolation.

RELATION OF THE APEX OF SOLAR MOTION TO PROPER MOTION AND ON THE CAUSE OF THE DIFFERENCES OF ITS POSITION FROM RADIAL VELOCITIES AND PROPER MOTIONS

By C. D. Perrine

OBSERVATORIO NACIONAL ARGENTINO, CÓRDOBA

Communicated by E. B. Frost, November 27, 1916

Continuing the investigation of the apparent dependence of the position of the solar apex upon proper motion as derived from radial velocities,[1] apices have now been derived from the proper motions

of the same stars which confirm the conclusions from the radial velocities. Solutions were made also from Adams' Mount Wilson list of 500 radial velocities[2] which show the same dependence. This list, however, contains no stars south of $-26°$.

The results from both sets of data are given in table I.

TABLE I

CLASS μ_α	RADIAL VELOCITIES			PROPER MOTION		P.M.—R.V.
	A	D	No. stars	A	D	ΔD
	degrees	*degrees*		*degrees*	*degrees*	*degrees*
2.9 and brighter. All spectral classes	258.0	+41.5	110	258.8	+24.6	−16.9
B	276.0	+29.6	193	272.8	+32.4	+ 2.8
A, F, G						
Large	268.1	+ 8.6	141	261.3	+22.1	+13.5
Medium	269.0	+ 7.0	81	238.5	+50.8	+43.8
Small	254.7	+35.0	277	263.5	+47.4	+12.4
K						
Large	288.7	+18.8	85	255.9	+13.0	− 5.8
Medium	250.4	+15.1	85	258.5	+42.9	+27.8
Small	260.4	+36.8	220	276.4	+69.2	+32.4
Mean	265.7	+24.0		260.7	+37.8	+13.8
Adams' 500 R. V.						
Large	276.9	+ 3.1	47	279.2	+38.4	+35.3
Medium	244.2	+41.6	32	249.6	+50.3	+ 8.7
Small	272.0	+31.6	349	266.4	+67.7	+36.1

These results also show systematic differences between the positions of the apex of the kind noted by Campbell[3] between his apex from 1193 radial velocities and that of Lewis Boss from 4686 proper motions. These discordances are chiefly in declination and are given below according to spectral class in the sense P.M.—R.V.

SPECTRAL CLASS	ΔD	SPECTRAL CLASS	ΔD
	degrees		*degrees*
B	+ 5.3	G	+22.1
A	+13.0	K	+14.7
F	+17.6	M	+ 7.1

There is some indication that the discordance is greater for the later spectral types, at least among the stars with small proper motion in right ascension, and possibly also some relation to magnitude and size of proper motion.

Asymmetry in the proper motions of the B stars and other peculiari-

ties prompted the making of separate solutions from northern and southern stars, the results of which are given in Table II.

TABLE II

PROPER MOTIONS

SPECTRAL CLASS μ_α	NORTH					SOUTH					ΔD
	A	D	X	Y	Z	A	D	X	Y	Z	N−S
		(all +)		(all +)	(all −)		(all +)		(all +)	(all −)	
2.9 and brighter											
All classes.....	260°.0	26°.0	+27.3	155.3	77.1	255°.4	21°.6	+ 26.6	101.8	41.6	+ 4°.4
B.....	281.5	46.4	− 3.3	16.3	17.4	269.4	26.2	+ 0.4	34.5	17.0	+20.2
A, F. G											
Large.....	258.0	35.6	+48.0	228.0	167.0	263.7	11.2	+ 39.0	356.0	71.0	+24.4
Medium.....	256.3	36.8	+ 6.3	25.8	19.9	227.9	(56.6)	+ 29.8	32.9	67.3	(−19.8)
Small.....	262.2	59.4	+ 2.4	17.2	29.4	264.5	40.1	+ 3.2	33.6	28.5	+19.3
K											
Large.....	255.4	17.9	+46.0	178.0	59.0	256.9	3.9	+ 31.0	131.0	9.0	+14.0
Medium.....	269.8	46.4	+ 0.2	58.9	61.8	245.5	35.9	+ 14.6	31.9	25.4	+10.5
Small.....	267.6	72.4	+ 0.5	12.2	38.8	282.2	66.2	− 3.2	14.7	34.1	+ 6.2
Mean.....		42.6					32.7				+ 9.6
Omitting ( ).....							29.3				+13.8
Adams'500 R.V.											
Large.....	279.8	33.6	−56.0	323.0	218.0	275.8	35.9	− 63.0	626.0	455.0	− 2.3
Medium.....	281.9	35.4	− 8.1	38.2	27.7	202.8	18.7	+115.2	48.5	42.2	+16.7
Small.....	279.4	70.7	− 1.2	7.2	20.9	230.9	50.9	+ 6.0	7.4	11.7	+19.8
π Obs. 0".06 and over.....	253.7	47.9	+97.0	332.0	383.0	258.0	24.9	+186.0	871.0	414.0	+23.0

These results disclose, if we omit one abnormal southern D, quite consistent differences similar to those between the results from radial velocity and proper motion for all portions of the sky. From these 1194 stars of the spectral classes B, A, F, G and K, the average of the apices from the proper motions of the southern stars agrees closely with the apices from the radial velocities of the same stars, but there is a difference of over 20° for the northern stars, as follows:

	D	
	NORTHERN	SOUTHERN
	degrees	*degrees*
Proper motions.....	+43	+29
Radial velocities.....	+21	+26
P.M.−R.V.....	+22	+ 3

An examination of the rectangular coördinates resulting from the solutions (Table II) shows that nine out of the entire twelve values of Y and all of the values from the small μ_α, are smaller for the northern than for the southern stars, indicating smaller average parallactic displacement for the northern than for the southern stars.

Taking the stars of type B as the best marked example, if we use the value of Y derived from the *southern* stars to determine D for the *northern* stars, we obtain essentially the same D for both northern and southern stars, a value which agrees well with that obtained from the radial velocities of the same stars and with that from the radial velocities of all of the spectral classes. This will be true in principle also for the other cases, but appears to be complicated in the later types by other factors.

Summary.—1. The position of the apex of solar motion depends upon the proper motions in right ascension of the stars used. The differences appear to be greatest in declination, the stars of large proper motion yielding apices south of those from stars with small proper motion. This effect is shown in the results both from proper motions and radial velocities.

2. The differences in D of the apices of solar motion as derived by other investigators from radial velocities and from proper motions are consistent and they appear in general to be greater for the stars of late than for those of early type. This discordance appears to arise chiefly from the proper motions of the northern stars and to be satisfactorily explained by the assumption that the parallactic displacement of the stars is systematically less in the northern sky.

The details of this investigation have been sent to the *Astrophysical Journal.*

[1] These PROCEEDINGS, 2, 1916, (376-378); *Astroph. J., Chicago, Ill.*, 44, 1916, (103-116).

[2] *Contrib. Mount Wilson Solar Obs.*, No. 105.

[3] *Berkeley, Lick Obs., Univ. Cal., Bull.*, No. 196, p. 128.

HYDROLOGY OF THE ISTHMUS OF PANAMA

By Brig. Gen. Henry L. Abbot

UNITED STATES ARMY, RETIRED

Read before the Academy, November 13, 1916

The Panama Canal being now opened to traffic, there remains for study only one important hydraulic problem—the sufficiency of the available water supply to meet the needs for lockage, for mechanical

power to operate the canal and railroad, and for the electric lighting of the Canal Zone. It should not be forgotten, however, that if a larger volume of water be desirable for these and other uses, the plan proposed by the New French Company to supplement the volume of its smaller projected lake is still available; namely, the construction of a masonry dam near Alhajuela, where a good site exists for creating an upper lake to hold back the surplus water which during the rainy months now runs to waste through the spillway.

The Canal Zone lies between the ninth and tenth degrees of north latitude in a region of exceptional rainfall; where the sun, closely followed by rain clouds raised from the oceans in his annual journey between the tropics, exerts a controlling influence upon the volume of local rainfall. When near his southern limit, in January, February, March and April, precipitation upon the Isthmus is at its minimum; December and May are usually intermediate in volume; in the remaining six months, when near his northern limit, heavy downpours are the rule. Furthermore, the local annual rainfall is not uniform across the Isthmus. As one passes from the Atlantic to the Pacific coast, the volume falls off gradually from about 130 inches at Colon to about 70 inches at Panama. It will be noticed that even the latter is more than double the usual downfall in the United States, a fortunate circumstance for our great artery of commerce. Another local advantage is the fact that the atmosphere of the Isthmus is nearly saturated with aqueous vapor, which largely reduces the losses by lake evaporation. Our hydraulic problem seems therefore to be specially concerned with Isthmian rainfall and outflow, and the relation between them.

To determine accurately the average annual rainfall at any locality, the records should cover at least half a century. Although the Isthmus has been known to civilization for more than four hundred years, the first annual rainfall records date from 1863, when they were begun by the agents of the Panama Railroad Company. As to the outflow from the watershed, the records are even less complete. The volume received from the clouds, after reduction by evaporation, by plant growth, and by possible infiltration, represents the available flow at the dam site. To determine to what extent existing data throw light upon this quantity, has seemed to me to be of primary interest.

The earlier records are given in a paper published in the *Monthly Weather Review* in May, 1899; they include those of the Railroad company, and those at that date collected by the two French Companies and the Liquidation. A gap of a few years in the former is supplied by a paper by Mr. A. P. Davis, published in the Twenty-second Annual

Report of the United States Geological Survey for 1900–1901. These data are supplemented and discussed by the writer in papers published in the *Monthly Weather Review* of February, 1904, and of June, 1908, together with some later data collected by the American engineers. In March, 1913, Mr. Caleb M. Saville contributed a valuable paper upon the Hydrology of the Canal to the Transactions of the American Society of Civil Engineers, bringing the records up to 1910. The Annual Reports of the Isthmian Canal Commission, of course, cover the American operations. The first point for consideration is, how these data can best be grouped for study?

The narrow limits to which the earlier observations were restricted suggest that the basin of the Chagres River above Bohio should be adopted until Gatun Lake began to fill early in 1910. Its area has been accurately determined by recent surveys to be 779 square miles. The early rain records were mostly restricted to Colon and Gamboa. Fortunately, Colon is situated near the Atlantic Coast line where the rainfall is largest, and Gamboa well represents the Pacific limit of the watershed. A careful analysis of the more ample records of recent years (1898–1907) has shown that the average rainfall in the basin above Bohio is about 89% of that at Colon, 124% of that at Gamboa, and 52% of the aggregate of the two stations. To avoid a change in the standard, these ratios have been used throughout the following table to estimate the annual rainfall in the basin; and where a few dry months are missing in the early railroad records the vacancies have been supplied by the mean values of the missing months.

The Chagres River is a torrential stream, and the first French Company early established a fluviograph at Gamboa to register continuously the heights of the water. This record has been carefully kept since 1883; until the rise of the lake in 1910 began to affect the local water level. At the times of freshets the oscillations are so sudden that their number and duration are readily noted, and they furnish valuable checks upon the discharge estimates. For this study they are assumed to begin and end at a stage 10 feet above low water, which is rarely exceeded.

Accurate measurements of discharge were inaugurated by the Liquidation and the New French Company, the continuous record dating from 1890. The annual outflow is given in the tables under the forms of cubic feet per second and depth in inches upon the watershed; the latter to permit a direct comparison with the rainfall. Years of great floods are also indicated, with the same object in view.

The first table is intended to present, for the basin above Bohio, all

available hydraulic data prior to the date when Gatun Lake began to fill, with some later figures added for comparison with those of the second table, which includes the entire watershed above Lake Gatun during the filling of the lake. In future studies this latter basin will doubtless be adopted, as the net outflow is easily determined and rainfall measurements are now made at many stations. For the second table I first[1] adopted the mean of the rainfall records at nine stations—Colon, Gatun, Trinidad, Camacho, Empire, Culebra, Gamboa, Alhajuela and El Vigia—as well representing the average volume falling upon the lake watershed near the Canal Zone. Seven new stations then unknown to me had been added, and the figures now given are taken from the noteworthy paper by Mr. Willson in the *Transactions of the International Engineering Congress at San Francisco.*

HYDROLOGY OF THE WATERSHED ABOVE BOHIO; 779 SQUARE MILES

YEAR	PRECIPITATION		IN THE BASIN ABOVE BOHIO			GAMBOA FRESHETS		AUTHORITY
	Colon Inches	Gamboa Inches	Rainfall Inches	Outflow		Annual No.	Duration Hours	
				Ft.-sec.	Inches			
1863	134.3		120					Panama Railroad Records
1864	123.4		110					
1865	107.4		96					
1866	129.7		115					
1867	120.8		108					
1868	120.0		107					
1869	114.8		102					
1870	149.6		133					
1871	99.6		81					
1872	168.5		150					
1873	87.1		78					
1874	137.7		123					
1875	94.7		84					
1876	124.5		111					
1877	115.5		103					
1878	86.7		77					
*1879	164.2		130					
1880	136.4		121					
1881	102.3	89.5	100					de Lesseps French Cie
1882	124.1	79.6	106					
1883	115.3	76.6	100			21	201	
1884	86.5	95.9	95			25	334	
*1885	146.3	97.5	127			37	437	
1886	137.2	102.9	125			43	437	
1887	154.9	136.2	151			46	709	
*1888		102.6	127			29	596	

* Great flood of the Chagres River.

HYDROLOGY OF THE WATERSHED ABOVE BOHIO; 779 SQUARE MILES—*continued*

YEAR	PRECIPITATION		IN THE BASIN ABOVE BOHIO			GAMBOA FRESHETS		AUTHORITY
	Colon Inches	Gamboa Inches	Rainfall Inches	Outflow		Annual No.	Duration Hours	
				Ft.-sec.	Inches			
1889		75.7						Collapse. Liquidation.
*1890	154.3	105.0	135	6,304	110.0	34	308	
1891	124.7	77.7	105	4,476	78.1	10	115	
1892	145.3	104.4	130	6,513	113.6	24	269	
*1893	131.9	117.8	130	7,081	123.3	12	220	
1894	153.7	90.6	127	6,098	106.4	25	263	
1895	151.5		135	4,482	78.2	7	54	The New French Cie
1896	131.5		117	4,216	73.5			
1897	138.0	107.8	128	4,830	84.2	16	157	
1898	115.5	82.6	103	3,944	68.8	8	51	
1899	133.0	80.0	111	3,384	59.0	8	57	
1900	116.1	78.7	101	3,509	61.2	12	84	
1901	107.7	91.6	104	3,855	67.2	13	107	
1902	112.6	97.7	109	4,179	72.9	5	37	
1903	126.3	99.3	117	3,958	69.0	12	94	
1904	126.9	84.5	110	4,110	71.7	13	94	U. S. in charge.
1905	115.4	82.2	103	2,800	48.8	6	48	
*1906	138.1	97.8	123	4,169	72.7	12	178	
1907	125.6	78.1	106	3,597	62.7	11	69	
1908	137.7	77.5	112	3,732	65.1	14	131	
*1909	183.4	122.1	159	7,335	128.0	27	466	
1910	150.0	116.0	138	6,340	110.6	15	77	Lake began to fill
1911	112.8	70.6	95					
1912	117.6	89.1	107					
1913	131.2	86.3	113					
1914	132.7	77.4	109					

* Great flood of the Chagres River.

HYDROLOGY OF THE WATERSHED ABOVE LAKE GUTUN; 1,320 SQUARE MILES

YEAR	LAKE ABOVE MEAN SEA LEVEL FEET	IN THE LAKE WATERSHED			LAKE EVAPORATION. FOOT-SECONDS
		Rainfall inches	Net outflow		
			Foot-seconds	Inches	
*1909	3.63	162.42	10,704	110.1	
1910	13.07	149.66	11,938	122.9	
1911	15.15	98.41	5,710	58.8	
1912	31.24	102.83	4,985	51.3	
1913	57.87	102.40	5,272	54.3	484
1914	85.26	100.54	5,118	52.7	704
1915	86.17	118.17	7,106	80.2	733

* Great flood of the Chagres River.

An inspection of these tables shows a satisfactory correspondence between the observed annual rainfall and the outflow and river oscillations as indicated by the number and duration of the freshets. There is, furthermore, a suggestion of a tendency to a progressive annual change between years of maxima and minima in these quantities, which is worthy of notice. If future records confirm this suggestion, it will be needful to study the reason for it, as is now the case with solar spots, aurora borealis, and other natural phenomena. As to the second table, it must not be forgotten that it covers a period when the area of the lake was gradually increasing, causing a variable loss due to evaporation. The values given for it for the last three years are taken from the official reports, and are based probably on direct measurements.

The tropical conditions of the Canal Zone are so different from those in the Continental United States that it is interesting to compare them. This may be done from Rafter's tables, in his valuable paper published as the Water-Supply and Irrigation Paper, No. 80, of the Geological Survey. It is done in the following table.

AVERAGE RAINFALL, RUN-OFF, AND DIFFERENCE

WATERSHED	AREA SQUARE MILES	PERIOD YEARS	RAIN-FALL INCHES	LOSSES			
				By Outflow		By Evap. Etc.	
				Inches	Per cent	Inches	Per cent
Muskingum River	5,828	8	39.7	13.1	33	26.6	67
Genessee River	1,070	9	40.3	14.2	35	26.1	65
Croton River	339	23	49.4	22.8	46	26.6	54
Lake Cochituate	19	38	47.1	20.3	43	26.8	57
Sudbury River	78	26	46.1	22.6	49	23.5	51
Mystic Lake	27	18	44.1	20.0	45	24.1	55
Neshaminy Creek	139	16	47.6	23.1	49	24.5	51
Perkiomen Creek	152	16	48.0	23.6	50	24.4	50
Tohickon Creek	102	15	50.1	28.4	57	21.7	43
Hudson River	4,500	14	44.2	23.3	53	20.9	47
Pequannock River	64	9	46.8	26.8	57	20.0	43
Connecticut River	10,234	8	43.0	22.0	51	21.0	49
Chagres River	779	21	118.1	82.1	69	36.0	31
Gatun Lake Basin	1,320	6	119.4	75.0	63	28.3	24

An inspection of the last four columns will show how much more favorable to the operation of a canal are the hydraulic conditions prevailing upon the Isthmus than those existing near the Atlantic Coast of the United States. Considerably more than double the volume of rainfall is available, and the losses from evaporation, plant growth,

and percolation are represented by only about 30% as against 50% of that volume. As stated above, it remains to be determined by the observations of many future years whether there is a tendency to a normal variation in annual rainfall upon the Isthmus, as seems to be suggested by the figures already of record.

[1] In a paper which appeared in *Professional Memoirs, Corps of Engineers, U.S.A.*, for November-December, 1915.

THE METEOR SYSTEM OF PONS-WINNECKE'S COMET

By Charles P. Olivier

LEANDER McCORMICK OBSERVATORY, UNIVERSITY OF VIRGINIA

Communicated by E. W. Morley and read before the Academy, November 14, 1916

The National Academy of Sciences, by two grants from the J. Lawrence Smith Fund, having been the means of greatly extending the membership and work of the American Meteor Society, it seems well to report to the Academy the most interesting single result of the work so far obtained.

Late in May and early in June of this year two of our observers, Mr. John Koep and Mr. Philip Trudelle, both of Chippewa Falls, Wis., sent in a series of observations which showed that meteors were at least three times as numerous as is usual for the time of the year mentioned. This attracted immediate attention and the observations were worked up without delay. The orbits, which corresponded to the positions found for the radiants, were then computed. As several of the orbits turned out to have the same elements, a search was next made to see if any of the known comets had a similar orbit.

At once it was seen that Pons-Winnecke's Comet fulfilled the conditions excellently and there was no doubt that the meteors and the comet had a common origin, and that another case of the connection between a meteor stream and a comet had been found.

The first orbits for the meteors had been computed on the usual assumption that they moved with a parabolic velocity, but there were obvious reasons to show that this was only a first approximation. The orbits were then all computed as elliptical, assuming their major axes equal to that of the comet. This set made a far better agreement with the elements of the comet's orbit and put the connection beyond question.

Before speaking further about the meteors themselves, it will be of interest to review briefly the history of this rather remarkable comet. Discovered by Pons in 1819, no further observations were obtained until

ELLIPTICAL ELEMENTS							PARABOLIC ELEMENTS								
No.	Log a	Log e	Log q	i	P	N	No.	Date	R A	Decl.	i	Log q	P	N	Obs.
1	0.514	9.857	9.962	16.7	280.1	60.7	1	May 21.6	224.5	25.2	20.2	9.954	279.7	60.7	J. K.
2	0.514	9.865	9.948	18.6	287.6	65.5	2	26.68	230.3	27.4	22.4	9.958	283.4	65.5	P. T.
3	0.514	9.854	9.970	17.6	282.6	66.1	3	27.25	231.0	27.5	22.5	9.958	284.2	66.1	J. K.
4	0.514	9.856	9.964	19.0	286.2	66.5	4	27.67	232.1	26.8	22.8	9.954	285.4	66.5	P T.
5	0.514	9.855	9.996	19.1	285.6	68.0	5	29.2	232.7	28.0	22.9	9.960	284.9	68.0	J. K.
6	0.514	9.852	9.974	17.6	287.6	73.2	6	June 3.7	234.4	27.5	22.2	9.965	288.4	73.2	J. K
7	0.514	9.855	9.967	18.2	291.6	74.2	7	4.68	235.8	25.6	22.0	9.961	291.0	74.2	P. T.
8	0.514	9.839	0.005	17.1	268.3	96.9	8	28.5	203.0	53.0	20.9	0.005	268.3	96.0	B
Comet	0.514	9.846	9.988	18.3	271.6	99.3									

1858 when it was rediscovered and named after Winnecke. Since then it has been seen in 1869, 1875, 1886, 1892, 1898, 1909, and 1915. While it has never been a bright comet, at its last return—a rather unfavorable one it is true—it never surpassed the twelfth magnitude in brightness and was more than a unit's distance from the earth. The comet belongs to Jupiter's family and at present has a major axis of about 6.52 astronomical units and a period of 5.89 years.

Since at certain epochs it has passed very near Jupiter, the most massive of the planets, the elements of its orbit have been changed in a most marked manner due to perturbations caused by that body. Briefly from 1858 to 1909 its longitude of perihelion has changed from 276° to 272°, its ascending node from 114° to 99°, the eccentricity of its orbit from 0.76 to 0.70, and finally its perihelion distance from 0.70 to 0.97 astronomical unit. This last change is the one of greatest importance for the present discussion, because it made possible the intersection of the earth's orbit with those of the meteors connected with the comet.

It will be seen that this element was slowly increasing during this interval of 51 years, and hence, if the progression continued in the same direction, in the seven years since 1909 it should have reached the value unity almost exactly, which means that the nodal point was indeed very near to the earth when the latter passed by it, late in June. While the inclination of the comet's orbit has increased from 11° in 1858 to 18° in 1909, still the extreme distances between the earth and the comet's 1909 orbit was only about 14,000,000 miles on May 25, and this decreased to 2,000,000 on June 28. Even this first figure is not excessive, because we already have positive proof that members of the Perseid and Aquarid streams have been observed at about that distance from the orbit of the parent comet.

The longitudes of the nodes differ considerably. This is due to the fact that this element of a meteor's orbit depends merely upon the position of the earth in its orbit, and when the agreement of the other elements is satisfactory, considerable differences in the node mean little. It will be seen that the other elements agree very well indeed, remembering the limit of accuracy possible in work on meteors, which is never comparable in this regard to results obtained with the telescope.

The data on which final conclusions were based are rather extensive. For America we had about 1100 observations reported by five of our members during the interval these meteors were seen, and three English observers also published results about the time of maximum, which are available. Eight orbits were calculated from this material, a number quite sufficient to illustrate the general agreement and order of accuracy of the work. Several others have since been computed. Of course in the final discussion all the data will be included and published in full detail.

While the working up of the American observations and the conclusions drawn from them are of course my own, yet I desire to make full acknowledgment to Messrs. Koep and Trudelle, through whose enthusiastic and excellent observing my part was made possible. Their work is of a high order, and it may be said that in 1916, from January to September, Mr. Koep sent in about 1800 observations and Mr. Trudelle about 1330.

It is only just to say that Mr. W. F. Denning of Bristol, England, observed these meteors and later published his observations, saying that the meteors moved in orbits somewhat similar to that of Pons-Winnecke's comet and they might possibly be connected. He gave no elements nor did he state on what sort of investigations his inference was based.

However, he published this before I sent my results to Harvard College Observatory, for publication in one of its *Bulletins*, but the journal containing Mr. Denning's results had not then arrived from England and I was ignorant of his conclusions.

Finally the members of the National Academy of Sciences will be interested to learn that since the first grant from the J. Lawrence Smith Fund became available last year no less than 5000 observations of meteors made by members of the American Meteor Society were sent in during 1915, and 7500 more were received up to October 1 of this year.

IMPROVEMENTS IN CALORIMETRIC COMBUSTION, AND THE HEAT OF COMBUSTION OF TOLUENE

By Theodore W. Richards and Harold S. Davis

WOLCOTT GIBBS MEMORIAL LABORATORY, HARVARD UNIVERSITY

Read before the Academy, November 14, 1916

The object of this investigation, which is part of a much larger program, was to secure further more precise knowledge of the heat of combustion of typical compounds of carbon, and further development of the methods of determination.[1] The work herein described followed directly after that detailed in the recent communication published with Dr. Frederick Barry; and the methods and apparatus resembled in most respects those already explained. Having profited by the experience of the earlier work, we were able to improve upon some of its details. Especial emphasis will be laid upon the improvements.

The method consisted in the successive combustion of toluene and a standard substance in the Berthelot bomb as modified by Atwater and Benedict, in oxygen under about 22 atmospheres pressure. The rise of temperature of the colorimeter containing the bomb was paralleled in the environment, so that no correction for cooling was needed. That this adiabatic method is capable of giving excellent relative results is shown by the series of investigation conducted at Harvard University; its absolute accuracy is proved by the recent careful work of the Bureau of Standards by H. C. Dickinson and his assistants.[2] The general assemblage of apparatus is adequately described and depicted in the most recent of the preceding papers, and the reader is referred to these as regards minutiae.

The details in which improvements were instituted were as follows: the mode of sealing the bomb, the mode of providing for the well regulated and complete combustion of the volatile substance; the mode of ignition; the automatic control of the temperature of the environment by a special 'synthermal regulator;' and the analysis of the residual gases for traces of unburned carbon monoxide. These several topics are discussed in order below, and finally the results for naphthalene and toluene are given. Other substances also were burned, but the details concerning these will be reserved for another communication.

The closing of the bomb.—The bomb was sealed by a washer of lead, sunk in a suitable circular slot and covered by a continuous round plate of gold foil (0.4 or 0.5 mm. thick) which protected the whole inside of

the cover of the bomb. The foil was soft enough to conform perfectly to the double rim of the lower part of the bomb, without breaking; the lead was soft enough to secure complete closure, and the gold wholly protected the lead. If the gold foil is pressed closely against the steel top of the bomb, there is little danger of its melting and if an accident happens, the foil is readily replaced. Accordingly, this method was uniformly used in the present research.

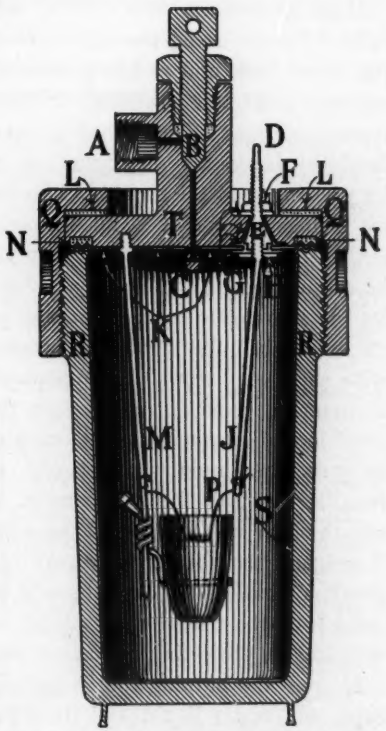


FIG. 1. NEW FEATURES IN CALORIMETRIC BOMB

DE, steel conical plug; *F*, steel nut holding plug in place; *G*, mica insulation; *H*, platinum nut protecting mica and clamping *J*. *J*, platinum rod for filing connection; *H*, gold lining to cover protecting lead washer *N*; *L*, phosphor-bronze ring to prevent friction of screw-cap *Q* on steel lid *T*.

The detrimental effect of friction of the steel screw-cap against the movable steel cover was greatly diminished by the use of a thin, smooth, flat ring of phosphor-bronze between these two parts. Thus rupture of the platinum lining was avoided, and the closure greatly facilitated.

The insulation of the wire for conveying the current necessary for the ignition was improved by making the conical piece (passing through the cover) of steel, not platinum, and insulating this from the cover by a thin layer of mica. A stout thread and nut, likewise insulated, on the outside, permits forcing the strong steel cone so firmly into place that the arrangement is wholly gas-tight. Into the base of this cone a thread is tapped for the stout platinum wire used to conduct the current, and the iron is protected by a wide platinum nut, which is insulated from the cover by mica. Thus the platinum wire is very firmly held in place and yet may be easily unscrewed, and the insulation of the steel cone remains undisturbed indefinitely. Figure 1 records all these improvements.

Other precautions concerning the makeup and treatment of the bomb were similar to those already described in previous investigations.

Mode of enclosing and igniting volatile liquids in thin-walled glass bulbs.—For enclosing the liquids to be burned, very thin bulbs, holding about 1 cc. and weighing about 0.4 g. were blown from a piece of ordinary soft glass essentially in the way illustrated in the preceding communication.[3] These must be much flattened on both sides to give flexibility (necessary on account of the pressure in the bomb), tested by pressing with the fingers. The volume of each bulb is easily found by immersion, and it is filled essentially in the way previously described. The bulb should not only yield under the pressure, but should be thin enough to burst easily. Otherwise the violent explosion might lead to incomplete combustion, and to injury to the apparatus. Typical very thin bulbs filled with benzene or toluene were found to burst with slight explosion at a temperature of about 50° in a water bath—a circumstance which may explain the occasional projection out of the crucible and incomplete combustion of some of the sugar at first used to ignite the liquid. Accordingly, instead of sugar about 0.1 g. of paraffin was used for this purpose, melting it upon the inside of the mouth of the weighed crucible by a hot glass rod outside of the crucible. A weighed fiber (about 0.5 mgm.) of cotton wool was attached to the paraffin, while heated for a moment locally by means of the rod applied outside of the crucible. A minute coil of very fine platinum wire enveloping the projecting cotton served to ignite it and the paraffin. The heat from the burning ring of paraffin ruptured the bulb through the expansion of the liquid, and the vapor of the liquid rose into a ring of flame at the mouth of the crucible and was completely burned. This method is so easy to adjust and is so convenient that it is a distinct improvement over the method formerly used. The result was of course corrected for the accurately known heat of combustion of the paraffin and shred of cotton wool.

Method of ignition.—Iron wire, heated by an electric current to start the combustion, has several disadvantages. The amount of iron burned and the stage of its oxidation are uncertain. Moreover, drops of fused oxide are sometimes driven against the platinum, and frequently destroy the crucible or the lining of the bomb. Very fine platinum wire, used by Zubow and Roth, is decidedly better. In detail the method used by us was as follows: 7–10 cm. of very fine platinum wire were wound in a spiral about 1 mm. in diameter, and attached at the ends of two supports directly above the crucible. As already stated, within the spiral was placed a weighed shred of filter paper or cotton wool,

dipping into the substance to be ignited first, whether this was sugar, napthalene or paraffin. A current of approximately 0.8 ampere ignited the cotton wool in about one-fifth of a second. The variations in the amount of heat obtained from the current in this time are wholly negligible in results like these, where one substance is measured by comparison with another treated in the same way.

The automatic control of the temperature of the environment.—In a recent communication from this laboratory an automatic device, or 'synthermal regulator,' for causing the environment around the calorimeter to match the temperature of the calorimeter itself, is described.[4] The present investigation was well in progress before this synthermal regulator was perfected, and in the meantime we had evolved a quite different much simpler device which served the present purpose sufficiently well, although not so generally useful as the other.

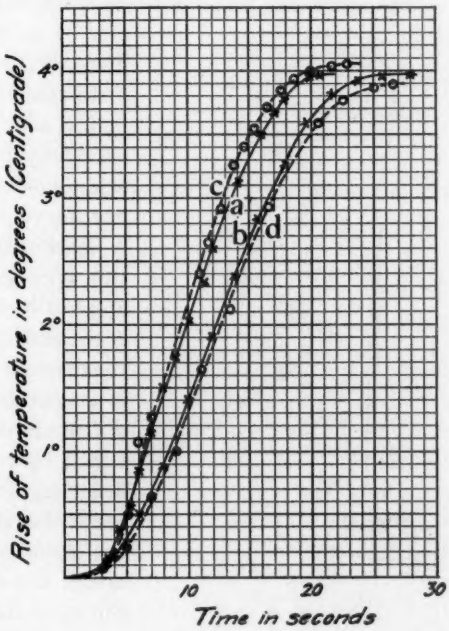


FIG. 2

Continuous lines *A* and *B* represent temperatures in calorimeter as indicated by thermometer in combustions of two different speeds.

Broken lines *C* and *D* indicate rise of temperature in environment as produced by apparatus to be described and illustrated in figure 3.

The general impression seems to exist that combustion within the calorimetric bomb is explosive and instantaneous.[5] According to our experience, this is by no means the case, especially with solid substances. We find that the rate of combustion is very variable, depending partly upon the nature of the substance, partly upon the state of aggregation, and partly upon the oxygen pressure. Thus benzoic acid or napthalene in powder, or a volatile liquid cause the temperature of the calorimetric system to rise very quickly when they are burned; but if the solids are compressed into hard tablets, the temperature rise is slow and steady and may require over three minutes for completion. The dif-

ference can only be accounted for by differences in the time actually required for the burning of the substances in the bomb. This conclusion is entirely in accord with that of Benedict and Fletcher,[6] based upon the measurement of the pressures developed during the combustion of solid substances.

The immediate practical problem was to match this rise in the calorimeter by an equal rise in the environment. Since the rise in temperature of the calorimetric environment, caused by adding sulphuric acid to the alkaline outside bath, is directly proportional to the amount of acid added, a device was needed for delivering very little acid at first, then rapidly increasing the amount, then for some time adding acid at a nearly constant rate, and finally diminishing the stream until it is reduced to nothing, in order to follow the carefully ascertained course of the rise of temperature of the calorimeter. These differences in rate of flow can easily be attained by constructing an apparatus for delivering acid automatically under different pressures. To attain the first part of the curve the level of the acid must run uphill; to parallel the straight line during the rapid rise, the liquid must be delivered from a larger reservoir of nearly constant level; to match the last part of the curve, it must rapidly sink in a fine delivery tube. The necessary dimensions of the several tubes were found by trial, and an apparatus like that shown in figure 3 was found to serve the purpose excellently. The tube to the right hand had to be long and spiral because its inside diameter could not be greater than 4 mm., on account of the small capillary constant of concentrated sulphuric acid.

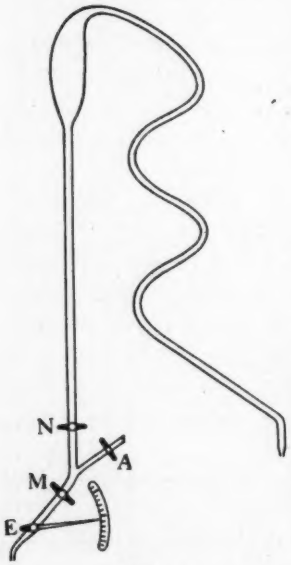


FIG. 3

A, stopcock for admitting sulphuric acid to apparatus in process of filling, *N* being open; *M*, stopcock for starting and stopping flow of acid into tank; *E*, regulating stopcock for determining rate of flow without altering general character of curve.

For each substance with each method one or two preliminary trials determined the general inclination of the curve. Previous experiments with alterations in the stopcock showed the inclination corresponding to each position on its scale. Therefore, for any particular substance, when all was ready for the combustion, the stopcock *E* had simply to

be set at the appropriate point, and at the moment of ignition *M* and *N* fully opened. The acid was then delivered automatically at a rate suitable for the case in hand.

Incomplete combustion.—In one of the earlier Harvard investigations the gases remaining in the bomb after the combustion of sugar were repeatedly analyzed for carbon monoxide or other volatile carbon compound, and none was found.[7] Nevertheless, with volatile liquids the danger is much greater, as is indicated by the careful work of Auwers, Roth and Eisenlohr.[8] The final test is the actual analysis of the resulting gases for combustible carbon compounds, especially carbon monoxide. This we have now carried out after many combustions, and can definitely report that with volatile liquids the combustion is often somewhat incomplete.

The arrangement of the apparatus was as follows: To the bomb, after each calorimetric combustion, was connected a train of tubes, as follows: 2 U-tubes filled with concentrated potassium hydroxide solution and glass pearls; a Liebig absorption tube containing saturated barium hydroxide solution; a drying tube filled with phosphorus pentoxide; a tube of copper oxide heated to a dull red heat by electricity; a tube of phosphorus pentoxide, and finally either a Liebig's potash bulb or a spiral filled with a definite quantity of tenth normal barium hydroxide, which was afterwards titrated.

In very many cases considerable quantities of carbon monoxide were thus found, often corresponding to errors of several tenths of a per cent in combustions carried out by the older methods, which seemed otherwise complete. This discovery entirely explains the discrepancy in previous results of many experimenters for benzene. Fortunately, it is only with volatile liquids of this sort that any important incompleteness of combustion was found.

On the other hand, the combustion was found to be fairly complete when volatile liquids were burned by the finally approved method of enclosing the liquids in very thin bulbs ignited by the ring of paraffin.

The gases from a combustion of benzene were also tested for oxidizable carbon compound by bubbling through a weak solution of alkaline potassium permanganate, and for ozone by allowing them to inpinge on starch-iodide paper. Both of these tests gave negative results.

Source and preparation of materials.—Napthalene was obtained from the Bureau of Standards; it had evidently been prepared by them with very great care, gave every evidence of having a high degree of purity, and serves admirably for standardizing calorimetric determinations. We found it much superior to sugar.

Toluene was prepared from commercial pure toluene by shaking with successive portions of sulphuric acid until the acid remains colorless. Further agitation with mercury until it had ceased to pollute a clean mercury surface, washing with water, and many times repeated distillation followed. The fraction which came over in the immediate neighborhood of 110.31° under normal pressure was retained as pure.[9] Several samples from different sources were employed.

There follows the record of the last three (and most successful) determinations of the heat of combustion of napthalene, chosen as a standard substance, and of three determinations of the heat of combustion of toluene, conducted in the thin sealed bulb with a rim of paraffin around the top of the containing crucible, as already described. Since they were thus all made over about the same range of temperature any uncertainty in the calibration of the thermometer largely disappears from the result—nevertheless this was carefully calibrated. These determinations serve to indicate the consistency attainable by use of the precautions just described.

Napthalene

NO. OF EXP.	WEIGHT NAPTHALENE	WEIGHT OF COTTON FOR IGNITION	SUM OF ALL CORRECTIONS	t_1^0	t_2^0	CORR. RISE OF TEMPERATURE
			degrees	*degrees*	*degrees*	*degrees*
5	1.1537	0.008	0.0220	16.008	20.202	3.616
6	1.1002	0.0095	0.0260	16.036	20.043	3.618
7	1.1023	0.008	0.0222	16.046	20.059	3.615
Average						3.616

Toluene.

NO. OF EXP.	WEIGHT TOLUENE	IGNITION MATERIAL	CORRECTION	CO FOUND	t_1^0	t_2^0	CORR. RISE OF TEMPERATURE FOR 1 GRAM.
			degrees		*degrees*	*degrees*	*degrees*
6	0.9154	Paraffin 0.0982 Cotton 0.0036	0.4256	None	15.961	19.885	3.822
7	0.9986	Paraffin 0.0953 Cotton 0.003	0.4143	2.0 cc.	15.553	19.782	3.820
8	1.0566	Paraffin 0.0747 Cotton 0.0079	0.3348	1.7 cc.	15.987	20.357	3.822
Average							3.821

Thus 1.0000 g. of napthalene in burning was capable of raising our calorimetric system by 3.616° and 1.0000 g. of toluene 3.821°, (or 1.0567 times as much as the same weight of napthalene) both substances having been weighed in air. According to the careful absolute determination of the Bureau of Standards 1 g. of naphtalene thus weighed evolves 9622 calories (20°) on burning. Hence 1 g. of toluene must evolve 10,168 calories, the toluene being weighed in air. Corrected to vacuum and to 18° this would become 10,158, a value only slightly less than that (10,166) found by Richards and Barry. A part at least of the difference may be due to the different standard of reference used in this case (napthalene instead of sugar). Evidently, the combustions in the earlier Harvard research must have been essentially complete, for such difference as exists is in the direction opposite from that which would have been caused by incompleteness of combustion.

Both results are much higher than that found by Roth and Auwers,[10] who apparently did not test the residual gases for carbon monoxide after combustion, and found the gram of toluene to give only 1.0529 times as much heat of combustion as a gram of napthalene, instead of 1.0567. It is not impossible that either all our toluene contained a slight aliphatic impurity[11] (a rather unlikely contingency because we obtained samples from various sources) or theirs contained traces of some other contamination,—since it is so very difficult to purify completely a compound of carbon, even by countless distillations, from substances with nearly the same boiling point.

Many other determinations of a variety of organic substances have been made here with the help of these improved methods and the matter will be pursued further, with especial reference to purity of materials. The results will be communicated in the near future in a publication of the Carnegie Institution of Washington, to which we are much indebted for generous assistance in this work.

Summary.—In this paper there are described improvements in various details of the procedure of calorimetric combustion, to wit: means of effectively closing the bomb with less risk of injury to the platinum lining and cover; means of burning volatile liquids without loss; a method of automatically controlling the temperature of the environment about the calorimeter so as to make the calorimetric operation more convenient and more truly adiabatic; and means of evaluating the incompleteness of combustion, if any volatile carbon compounds should remain unburned. New determinations of the heat of combustion of toluene are recorded, giving the value 10,155 calories (18°) per gram (weighed in vacuo). This is but a preliminary publication.

[1] For references, see Richards and Barry, *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (993–1020).

[2] *Washington, D. C., Bull. Bur. Stand.*, **11**, 1914, (243).

[3] Richards and Barry, Loc. cit.

[4] Richards and Osgood, *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (1718–1720).

[5] Stohmann, *J. prak. Chem., Leipzig*, **39**, 1889, (514).

[6] Benedict and Fletcher, *J. Amer. Chem. Soc.*, **29**, 1907, (739–757).

[7] Richards, Frevert, and Henderson, *Boston, Mass., Proc. Amer. Acad., Arts. Sci.*, **42**, 1907, (584).

[8] Auwers, Roth, and Eisenlohr, *Liebig's Ann. Chem., Leipzig*, **385**, 1911, (102–116).

[9] Richards and Barry, *J. Amer. Chem. Soc., Easton, Pa.*, **36**, 1915, (997).

[10] Roth and Auwers, *Liebig's Ann. Chem., Leipzig*, **407**, 1914, (154, 158).

[11] For example, 2-4 Dimethyl hexane, boiling point 110°, L. Clarke, *J. Amer. Chem. Soc., Easton, Pa.*, **30**, 1908, (1148). Octanes have higher specific heats of combustion than toluene. (Richards and Jesse, *Ibid.*, **32**, 1910, (292).

THE MASS OF THE ELECTRIC CARRIER IN COPPER, SILVER AND ALUMINIUM

By Richard C. Tolman and T. Dale Stewart

Communicated by R. A. Millikan, November 28, 1916

In a previous article [*Physic. Rev.*, **8**, 97 (1916), these PROCEEDINGS **2**, 189 (1916)] we have described some experiments in which a coil of copper wire was rotated about its axis at a high speed and then suddenly brought to rest, the ends of the coil being connected with a sensitive ballistic galvanometer which permitted a measurement of the pulse of current which was produced at the instant of stopping by the tendency of the electrons to continue in motion.

We have continued these experiments making use of three new windings of copper wire, and using two different windings each of silver and aluminium wire. These further experiments were made, not only because it seems desirable to subject so new a phenomenon to a more rigid test, but because it is also desirable to see if the mass of the carrier of electricity is the same in all different metals and how much it differs, if at all, from the mass of the electron in free space.

We now have a record of the results of 624 individual runs made on a number of different coils, using three kinds of wire, two different sizes, and two different kinds of insulating binder to hold the coils in place. The runs were made with various total resistances in the circuit, with various lengths of wire, and at various velocities, rotating sometimes in one direction and sometimes in the other. Not only was the pulse of electricity every time in the direction which would be predicted on the basis of a mobile *negative* electron as the carrier of electricity,

but the experiments have led to concordant results for the mass of this carrier. In free space the mass of the electron may be taken as 1/1845 times that of the hydrogen atom, while we have found for the carrier in copper 1/1660, in aluminium 1/1590, and in silver 1/1540. We hope to construct a new apparatus which will increase the accuracy of measurement enough so that we can make certain whether the mass of the carrier is really larger in metals than in free space.

A more complete account of these experiments has been accepted by the *Physical Review* for publication.

THE SILVER VOLTAMETER AS AN INTERNATIONAL STANDARD FOR THE MEASUREMENT OF ELECTRIC CURRENT

By E. B. Rosa and G. W. Vinal

U. S. BUREAU OF STANDARDS, WASHINGTON, D. C.

Read before the Academy, November 14, 1916

The International Electrical Conference which met in London in 1908 adopted the ampere as the second fundamental electrical unit, the ohm being the first, and defined the international ampere in terms of the electrolytic deposit of silver in the silver voltameter. At the time of this conference it was the opinion of the delegates from this country that the volt should have been chosen in place of the ampere, because the standard cell was more reproducible than the silver voltameter and was the means then as now actually employed (in conjunction with the ohm) for measuring the ampere by the drop in potential method. The decision of the conference was, however, accepted as final, and researches were undertaken in several different countries, and particularly in this country, with the aim of making the voltameter worthy to bear the responsibility imposed upon it by the London Conference. The purpose of this paper is to give briefly the most important results that have been obtained and to show the remarkable agreement of the measurements recently made in the national laboratories of several different countries of the electromotive force of a Weston normal cell, in terms of the international volt as officially defined. This agreement is due to the fact that great advances have been made in our knowledge of the silver voltameter in recent years; and although no adequate specifications have been formally adopted, the methods followed by recent investigators have agreed in essential particulars, although differing in details.

No concrete standard for the ampere, corresponding to the column

of mercury for the ohm or the standard cell for the volt, is possible. The value of an electric current may, however, be conveniently expressed for the purposes of precise measurement in two ways. Either the silver voltameter may be used to calibrate an ampere balance as was done by the British Board of Trade, or the voltage of the Weston normal cell may be determined when a measured current passes through a known resistance. The latter method was adopted by the London Conference, but at that time information was lacking as to the proper value to assign to this cell, which was adopted by the conference in place of the Clark cell.

In 1910 a committee consisting of representatives of the National Laboratories of England, France, Germany, and the United States met in Washington and made careful voltameter measurements to determine the voltage of the Weston normal cell, in the manner agreed upon at London in 1908. The Committee found the value to be very nearly 1.0183 volts at 20°C. This value, recommended by the Committee as the international value of the Weston normal cell, has become the basis of measurement in general use for voltage and (with a resistance) for current measurements also. Although the cell has thus become the practical standard, the voltameter has not lost its importance, for it is the ultimate standard which is to be depended upon in the future to determine whether the standard cells preserved by the various national laboratories are maintaining an unchanging voltage.

The researches on the voltameter which have been made since the international technical committee adjourned have been in part for the purpose of finding out with greater precision (to the sixth significant figure) how close the value for the cell as computed from the voltameter results and the international ohm is to the value 1.0183 volts adopted by the Committee. Although the difference is inappreciable for most purposes, it is desirable that it be determined as accurately as possible. It is also very necessary to have adequate specifications for the voltameter that shall enable us to obtain results in the future comparable with those obtained now. In this work the Bureau of Standards has been able to take an active part. Many sources of error in the voltameter have been discovered and means for their elimination provided, and the voltameter may now be considered capable of being used to check the voltage of the Weston normal cell.

During the course of the work differences of opinion have arisen among some of the investigators in this and other countries, but by co-operative experiments and correspondence practically all of these differences

have been settled so that at present, we believe no important questions are still outstanding. A few of the most important developments during the recent voltameter work may be mentioned.

It was well known before the Bureau of Standards began its work that the deposits in the Rayleigh or filter-paper form of voltameter exceeded those in the porous cup voltameters which were used in series with them. No satisfactory explanation of this difference had been given, and its existence had indeed been questioned. The most recent investigation prior to the London Conference appeared to show that under certain conditions the two forms of voltameter agreed. It was discovered at the Bureau of Standards that the difference could be doubled if the amount of filter paper was doubled and that when the filter paper was wrapped around the porous cup the results were the same as if the porous cup were not present. In short, it was definitely proved that the filter paper itself was a source of serious error in the voltameter. A chemical study showed that the cellulose fibers of the filter paper are slightly oxidized in the air and the minute amounts of oxycellulose are soluble in water. These give rise to strong reducing agents in silver nitrate solutions and colloidal silver is formed. The colloidal particles carry electrical charges much smaller proportionally to their mass than do the univalent silver ions and therefore give rise to deposits in excess of the true deposit according to Faraday's law. They also alter the form and appearance of the deposit.

Methods have been developed at the Bureau for the preparation and testing of silver nitrate of the exceptional purity which is necessary for voltameter work, because very minute impurities produce exaggerated effects in the voltameter deposits. All forms of voltameter (except the filter paper form and forms using linen, cotton, silk, etc., as a septum) are in substantial agreement when used with solutions of the highest purity and with proper precautions against anode slime. The temperature coefficient with pure solutions was shown to be zero as it ought to be. The precaution taken by some observers to soak the silver deposits over night in distilled water to remove the last traces of electrolyte was shown to be harmful because it was discovered that silver in contact with platinum is appreciably soluble in distilled water. This was shown by repeated tests. The silver and the platinum differ slightly in potential so that a current passes from the silver to the platinum through the water. Although this current is very small, it was demonstrated by a sensitive galvanometer and the loss of silver from the deposit was measured.

Another discovery of importance is the alloying effect of the silver and platinum when determining the inclusions of foreign matter in the deposits by the method of heating. After the slight alloying has taken place, the removal of the silver leaves a very thin layer of platinum black on the inside surface of the cup. Unless this is entirely removed its adsorptive properties make the weight of the cup uncertain and its catalytic properties cause the deposition of hydrogen ions when another silver deposit is made so that the amount of silver is not a true measure of the electricity according to Faraday's law.

Various observers in different countries by avoiding these sources of error have obtained very concordant results as the following table shows:

TABLE 1

VALUES FOR THE WESTON NORMAL CELL AT 20°C. COMPUTED FROM SILVER VOLTAMETER MEASUREMENTS

COUNTRY*	FORM OF VOLTAMETER†	VALUE FOR CELL	Δ
			parts in 100 000
United States	Smith	1.01827_4	-0_2
United States	Richards	1.01826_7	-0_9
Japan	Smith	1.01826_9	-0_7
Russia‡	Smith	1.01829_5	$+1_9$
Holland	Smith	1.01826	-1_8
Germany	Kohlrausch	1.01829_0	$+1_4$
Mean		1.01827_8	$\pm 1_1$

* England and France have not published any measurements of this kind since the time of the International Committee. The Observers are: for Japan, Obata; for the U. S., Rosa, Vinal, and McDaniel; for Russia, Mlle. Ferringer; for Holland, Haga and Boerema; for Germany, von Steinwehr.

† The Smith form and the Kohlrausch form are similar in principle. They have no separation between anode and cathode except a glass trap to catch the anode slime, but the construction of this trap is very different in the two forms. The Richards form makes use of a porous cup septum.

‡ This is the mean of four sets of measurements on a particular cell at different periods during several years.

The average deviation from the mean result is only 0.001%. The experimental error involves the errors of both the cell and the standard resistance as well as the numerous sources of error in the voltameter itself. These include the errors of weighing the bowls both before and after the deposit, the errors of timing the deposits, the errors due to fluctuation in the current, and the errors in washing and drying the deposits. In view of these it is remarkable how small the deviations are for the individual countries. These results are the work of investigators

of five nationalities using three different types of instruments, and yet they agree to about 1 part in 100,000.

The national laboratories were in correspondence on the matter of specifications when the war in Europe began. Since then the Bureau of Standards has prepared and published its specifications for the voltameter and it will recommend these for adoption when it is again possible to do so after the restoration of peace.

The specifications which the Bureau of Standards has formulated have been made as broad as is consistent with work of the highest accuracy. They do not specify any particular form of voltameter, but rather the conditions which must be fulfilled by the voltameter. The concentration of the solution is given rather wide limits because it has been found that identical results may be obtained with pure solutions over a considerable range.

An important question as to the purity of the silver obtained has only recently been settled. The impurities are very small, as indeed previous observers, including Van Dijk in Holland, Jaeger and von Steinwehr in Germany, and Boltzmann in Austria, have previously shown. The Bureau of Standards has found the average amount of foreign matter in its standard deposits to be about 0.004%. It is so uniform in the different deposits that the results given by the voltameter as a measuring standard are sufficiently accurate without the necessity of determining the foreign matter in each individual deposit. Without the greatest precautions the error introduced in attempting to determine the inclusions will be greater than the total amount of the inclusions.

In some of the experiments at the Bureau of Standards our absolute current balance has been used in connection with the silver voltameter. This has permitted the determination of the absolute electrochemical equivalent of silver. It was found to be 1.11804 mg. per coulomb, but when corrected for the inclusion of foreign material this becomes 1.11800 mg. This figure is exactly the value assigned to this constant, although at the time it was adopted by the London Conference, eight years ago, the best information available indicated a higher value by about 30 parts in 100,000. In Holland, Prof. Haga, has obtained 1.11802 using a tangent galvanometer. We believe that the closest figure that can be assigned at the present time to this constant is the round number 1.11800 mg. per absolute coulomb, and is this precisely the value fixed by international agreement in 'international coulombs.'

Since one absolute coulomb of electricity deposits 0.00111800 gram of silver, the number required to deposit a gram-equivalent of silver,

107.88 grams, is 96,494 coulombs, which is the value of the Faraday. For general use the value 96,500 coulombs is a convenient round number which is within the experimental error.

Although some phenomena of the voltameter are not yet perfectly understood, the recent researches have shown how to use it as a reliable current standard and as a means of checking the constancy of the value of the Weston normal cell. The experiments made at the Bureau of Standards during the past eight years have been fully described in a series of papers which have appeared in the *Bulletin* of the Bureau.